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CONTENTS

An Examination of Selected Aspects of the Continuity and Noncontinuity Positions in Discrimination Learning. <i>Kenneth P. Goodrich, Leonard E. Ross, and Allan R. Wagner</i>	105
Some Aspects of Value in Psychology and Psychiatry. <i>Henry Winthrop</i>	119
Flesch Formulas Applied to Current Readings. <i>Zietta S. Pace</i>	133
Situational Determinants of Affective Reactions to Persons. <i>Boice N. Daugherty</i>	139
An Emmert's Law of Apparent Sizes. <i>G. Richard Price</i>	145
The Functional Role of Discriminative Stimuli in Free Operant Performance of Developmentally Retarded Children. <i>Robert Orlando</i>	153
Is the "Click" a Token Reward? <i>Robert C. Bolles</i>	163
The Conditioning of Fear to Internal Stimuli. <i>Max L. Fogel</i>	169
Relative Effects of Drive Level and Irrelevant Responses on Performance of a Complex Task. <i>Philip A. Marks and Norris Vestre</i>	177
Perspectives in Psychology: XVII. Interrelations of Fact and Value in Science. <i>P. E. Lichtenstein</i>	181
The Interaction Between Critical Flicker Frequency and Acoustic Stimulation. <i>Edward L. Walker and Thomas M. Sawyer Jr.</i>	187
Book Reviews.....	192
Books Received.....	207

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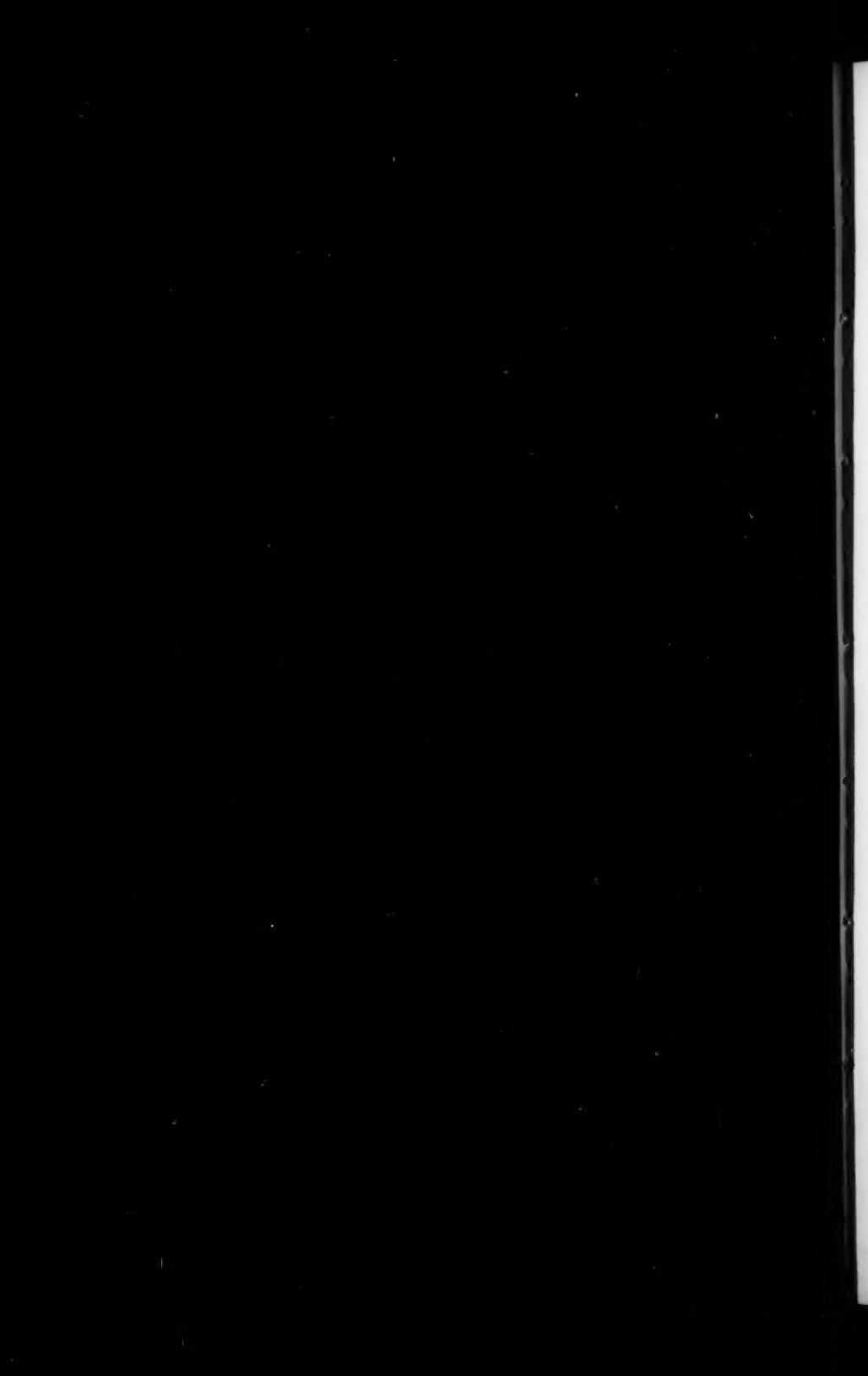
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AN EXAMINATION OF SELECTED ASPECTS OF THE CONTINUITY AND NONCONTINUITY POSITIONS IN DISCRIMINATION LEARNING¹

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The present paper is an attempt to clarify several issues involved in the so-called continuity-noncontinuity controversy in discrimination learning. No attempt is made to present a comprehensive review of the literature. Instead, an examination is made of several important aspects of this controversy which have not been made explicit and which may be continuing sources of misunderstanding in the continuing analysis of discrimination learning. Of particular importance are the "discriminably different proximal stimulation" aspect of the continuity position, and the evolution and status of certain "perceptual" features of the noncontinuity position. In addition, the relevance of the design and interpretation of two relatively recent studies (Lawrence and Mason, 1955; Goodwin and Lawrence, 1955) for the continuity-noncontinuity controversy is critically examined.

Background and Initial Debate

In an early paper, Lashley (1929) noted that in a brightness discrimination situation it was common for the animal to respond with position habits, alternation behavior, or to cues from the experimenter's movements *before* responding systematically to the relevant visual stimuli. Combined with the observation that following these "attempted solutions" there occurred a sharp drop in the error curve, these facts led Lashley to characterize the discrimination habit as a "single association acquired only after a number of familiar solutions have been tried unsuccessfully," with both the practice preceding and the errors following the solution being irrelevant to the actual formation of the association (Lashley, 1929, p. 136).

These comments provided the impetus for a series of studies by Krechevsky (1932a, 1932b, 1933a, 1933b) from which the continuity-noncontinuity issue developed. Recording not only the error scores but also the position of the chosen alley in a Stone multiple-unit discrimination box, Krechevsky showed that even while the error score with respect to the relevant stimuli was at 50% ("chance") during the

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initial stages of learning the Ss actually were responding systematically on some other basis (e.g., left-going, or alternation) as defined by a response level significantly exceeding chance ($50\% \pm 3$ sigma). In other words, the apparent randomness of presolution behavior was an artifact of using only the error score with respect to the correct solution as the behavioral measure. To these systematic responses he gave the name 'hypotheses' or 'hypothesis behavior.' While Krechevsky recognized that these terms might carry with them "mentalistic" implications, he considered the terms unobjectionable following the assignment of operational definitions to them. Setting aside this question of descriptive vocabulary, it should be pointed out at this point that the experimental evidence itself was relatively clear. The period before the solution is marked by the occurrence of statistically-defined systematic responding. The remaining problems from the point of view of this paper concern attempts at explanation of these and related phenomena.

In 1936, Spence presented a theory of discrimination learning based on stimulus-response and association principles. This formulation attempted to explain the process of discrimination learning in terms of gradually increasing excitatory strengths of positive stimuli as a result of reinforcement of responses to them, and gradually decreasing excitatory strengths of negative stimuli as a result of certain inhibitory effects of nonreinforcement. A further hypothesis, of central importance for the controversy under consideration, was presented by Spence. The strengthening and weakening processes were assumed to occur whenever the cue stimuli were received on the S's "sensorium" in the proper temporal relationship with the response. Thus as long as stimulation was received in this way, the learning process was "continuous." Spence (1936) recognized and Krechevsky (1937) concurred that there was no necessary conflict between this theoretical analysis and Krechevsky's largely descriptive formulation.

Opposed to Spence's conception outlined above, which has come to be called the *continuity* theory, is the noncontinuity position. Spence (1936) characterized the latter position as one which holds that the S solves a problem by trying one solution after another, from its repertory of hypotheses, until the correct one is hit upon, and that the S is quite unaffected by the relevant cue aspect until the time of correct solution. Thus the noncontinuity position would maintain that differential response tendencies to the relevant cues are not acquired during the period when S has not yet hit upon the correct hypothesis, while the continuity position would maintain that the learning process represents cumulative changes occurring during the presolution period, providing that the cue stimuli are impinging on S's sensory surface at or near the critical moment of response. These alternative formulations described by Spence, which we shall refer to as the "classical" continuity and noncontinuity positions, are built upon one important condition. It is assumed that the terms 'hypothesis,' 'systematic response tendency,' and

'presolution period' may be objectively defined so as to preclude uncertainty concerning their presence or absence. We shall see that this requirement plays a large role in the development of the controversy.

As an experimental test of the two opposing viewpoints, Spence (1936) suggested a discrimination experiment in which the stimuli which are to be positive and negative during the learning phase are reversed during the presolution period, *i.e.*, before the *S* begins responding systematically to the relevant stimuli. According to the noncontinuity theory the initial training should not result in any slower learning of the main problem when compared with a control group which did not receive the reversed initial training.² The continuity theory, on the other hand, would predict a retardation of the learning of the main problem.³ A study by McCulloch and Pratt (1934) utilizing weight discrimination was essentially the same as Spence's suggested experiment. These investigators found that the group in which the cues were reversed before the *Ss* were responding more than 50% to the positive cue required a greater number of trials and made more errors to learn the main problem than a control group which never had the reversed problem. The results seemed to substantiate the classical continuity prediction. Krechevsky (1938), however, reported a similar experiment with visual discrimination, the results of which were at least in part contrary to the findings of McCulloch and Pratt. Using stimuli consisting of horizontal or vertical rows of black squares on a white background, Krechevsky gave one group 20 trials with the cues reversed before beginning training on the standard problem, while another group was given 40 such reversed trials. Both of these groups were compared with a control group for rate of learning the standard problem. While the 40-trial group showed a retardation in the learning of the standard problem as would follow from the continuity position, the 20-trial group did not differ from the control group.

Development of the Continuity Position

The proponents on both sides of the controversy offered a number of possible explanations for the ambiguity of the results in the Krechevsky experiment. Two of these proposals are of particular interest, and we shall analyze them in considerable detail here and in the following section. The continuity position had to explain the performance of Krechevsky's 20-trial group which appeared to yield noncontinuity results. Spence (1936) had pointed out prior to the Krechevsky paper

2. The noncontinuity formulation might, in fact, be expected to predict that the reversal pretraining would facilitate performance on the main problem, since such training would permit *Ss* to discover the inappropriateness of such alternative hypotheses as position, alternation, etc. That this prediction was never made is probably attributable to the vagueness of the noncontinuity theory as to what constitutes the failure of an hypothesis and how such failure would lead to the abandonment of that hypothesis and the trying out of another.

3. It should be pointed out here that while Spence made specific assumptions as to the relative amounts of strengthening and weakening which the *S-R* tendencies undergo, for this particular test it is necessary only to assume that there is *some* change in the tendencies during the pre-solution period.

that in order for the learning to occur it is necessary for the cue stimulus to impinge upon the S's "sensorium" in temporal contiguity with the response. He had also observed (Spence, 1937) that in some situations it is necessary for the S first to learn certain receptor exposure or orienting acts before the cue stimulus can be received. On the basis of these observations, Spence (1940) proposed for Krechevsky's 20-trial group that the relevant cue aspects, rows of black squares, simply were not seen at first because the Ss were fixating other features of the stimulus situation. It is quite possible that in a situation requiring *form* discrimination, such as Krechevsky's, the S must first learn to make appropriate head and eye movements before the relevant stimuli are received on the retina. Prior to the learning of these receptor-orienting acts there could be no change in the associative connections between the response and the relevant stimuli. In such a case Spence maintained that a continuity theory would expect the obtained results, *i.e.*, the 20-trial group, would show no effects of cue reversal, whereas the 40-trial group, which was presumably allowed the opportunity to learn to fixate the form, cues before the reversal, would exhibit a retardation in the learning of the standard problem.

It is clear that there are practical technical problems involved in ascertaining whether and how receptor-orienting acts are involved in any particular learning situation. Lashley (1942) asserted that there were problems of a more fundamental nature involved. He pointed out that a geometrical analysis of the relationships between animal and apparatus in the Krechevsky experiment showed that the relevant stimulus cues *were* present on S's retina and that, therefore, the Ss were receiving the relevant cues from the beginning of training. This meant for Lashley that Spence's argument about the Ss not "seeing" the cues could only be construed as asserting that the Ss were not "perceiving" the cues, or did not have the requisite "set." If Spence did intend to imply that more than observable receptor and postural adjustment must be considered when he held that Ss "attention" was an important factor in discrimination learning, then the two theories were actually talking about the same thing.

In answer, Spence (1945) argued that the cues must not only be projected onto the receptor surface, but these projections must be "discriminably different." Spence's analysis of the reversal situation consisted, then, of two assertions: (a) if differential response tendencies are to be built up during the presolution period, it is necessary that the S received "discriminably different proximal stimulation," and (b) in order for this to be the case in some experimental situations, it may be necessary for S to learn certain receptor-orienting responses. The implications to be drawn from this dual assertion depend critically upon the meaning of the phrase "discriminably different proximal stimulation." If, for example, the discriminability of the proximal stimulation

depends upon such factors as "perceptual organization" or "set," then the rival theories may be indistinguishable.

We commonly refer to cues as being discriminably different for some organism only when he responds differentially to them, *i.e.*, the relation between the cues called "discriminably different" must be defined by differential response. In the present context, this may be accomplished by taking 'discriminably different' to refer to the condition that if Ss (of such and such a kind) are given differential reinforcement on these cues (in such and such a situation), these Ss can learn to respond differentially to them. In the light of this definition, which is admittedly only an outline, we may analyze Spence's requirement that in order for differential response tendencies to be built up to stimulus cues at a given point in training the S must receive the cues as "discriminably different proximal stimulation." This is interpreted to mean that, given the particular Ss, apparatus, and specific receptor-orienting behavior of the Ss, we must be able to establish that these Ss could, if given differential reinforcement on these cues in this apparatus *with these orienting behaviors present*, eventually learn to respond differently to the cues in question. Thus if the Ss in the reversal experiment show the same orienting behavior throughout the entire experiment and if they eventually learn the test-phase discrimination problem, it can be asserted unambiguously (by definition) that the Ss were receiving discriminably different proximal stimulation during the presolution period. In the case of Krechevsky's 20-trial group, Spence claimed that some change in orienting behavior occurred between the presolution period and final learning, and that indeed this change was necessary if the Ss were to be able to learn the problem. The retinal cues which were present initially—those which Lashley pointed out—were not adequate for the learning of the problem.

Since the problem of orienting acts and their associated retinal patterns may be eliminated when the experimental situation does not require the learning of specific receptor orienting acts, Spence suggested that further tests of the two positions be restricted to such situations. The McCulloch and Pratt (1934) experiment thus remained a satisfactory test of the continuity position, as did a brightness discrimination problem (Spence, 1945) with alleys of different brightness. Ehrenfreund (1948) showed that under special circumstances a form discrimination situation may satisfy the stimulation requirements. In general, however, difficulty in specifying in form and pattern discrimination situations the point at which discriminably different stimulation is first being received makes doubtful their usefulness in evaluating the continuity position.

Development of the Noncontinuity Position

It should be noted that the continuity workers discussed in the previous section were concerned with what we have termed the classical

issue, *i.e.*, with the question of whether during a behaviorally-defined presolution period the Ss were "learning about" the relevant cues. The tests which they offered as being appropriate were basically the same as the original experiment by McCulloch and Pratt (1934). There had been added a greater awareness of the importance of receptor exposure processes.

We turn now to the non-continuity formulations. In 1938 Krechevsky, who up until this point had not taken part in the controversy over interpretation, came out in support of the noncontinuity position. When we examine his 1938 paper, however, we find that his noncontinuity views differed from the classical position. He maintained, for example, that one must know the "actual" length of the presolution period and that the number of "presolution" trials given to his 40-trial group were probably too many for the results to be regarded as conclusive. Moreover, he went on to speculate that such factors as "shifts in attention" may determine whether the reversal pretraining would have any effect on the subsequent learning. That such a position implies a turning away from the definition of 'hypothesis' and 'presolution period' in terms of presence or absence of systematic responding seems clear. As both McCulloch (1939a) and Spence (1940) pointed out, there could be no such questions as Krechevsky raised if one were to retain the original behavioral definitions. One simply would have to look at the data to see whether the Ss were or were not responding systematically to the particular stimulus aspects.

The position which can be abstracted from Krechevsky's statements above is that systematic responding may be the *manifestation* of an hypothesis, but is not the *definition* of 'hypothesis.' Moreover, even when an S is in the presolution period (behaviorally defined) and has no hypothesis (behaviorally defined) about the relevant stimuli, he *may* or *may not* learn anything about the correct cues depending upon such factors as "shifts in attention," etc. It should be clear that this is quite different from the classical noncontinuity position which would state that when an S is in the presolution period (behaviorally defined) and has no hypothesis (behaviorally defined) about the relevant stimuli, he *does not* learn anything about these cues. The change in the non-continuity position is made more explicit in the writing of Haire (1939), who asserted that an hypothesis is not the systematic responding itself, but must be inferred to precede such systematic responding. Thus in Krechevsky's 1938 experiment the group which showed negative transfer was simply a case in which the hypothesis about the relevant cues had already "begun," but in which the behavior had not shown it as yet. Haire stated that, "It seems clear that a too strict operationism clouds the issue" (Haire, 1939, p. 300).

If methodological confusion is not to prevail, this statement by Haire must be examined. We note first that Krechevsky apparently

realized that the danger of indeterminateness was involved in the application of such terms as "set," "perceptual organization," "awareness of," "hypothesis," etc., when he originally advocated definition of 'hypothesis' in terms of systematic responding. The shift away from this position by Krechevsky and Haire may be viewed logically as the abandoning of a defined concept which did not have the specific usefulness or significance sought for it. Thus Haire's dismissal of "operationalism" could be viewed as such a rejection of a particular operational definition for the term 'hypothesis.' It may also be understood as a recognition that concepts from phenomenological introspection and prescientific discourse may guide the scientist in his efforts toward a more rigorous treatment of a subject matter. These possibilities are, of course, entirely compatible with operational analysis. Indeed progress in many scientific areas has been characterized by the re-definition of concepts in order to arrive at more useful or significant formulations. The important question here, however, becomes one of whether some satisfactory substitute (*i.e.*, a formulation leading to testable predictions) was offered following the rejection of the systematic responding definition of 'hypothesis.' As we proceed to trace further the noncontinuity position we will see that such a substitute definition appears to be lacking.

Haire's position implicitly rejected the reversal design as an appropriate test for the continuity-noncontinuity issue, and both McCulloch (1939b) and Spence (1940) indicated that they saw the Haire position as too indeterminate to test. Lashley (1942), however, further elaborated the new noncontinuity position and carried out certain experiments to test it against the continuity formulation. Three separate "postulates" appear to occupy a central position in Lashley's schema. *First* is the noncontinuity assertion that an animal cannot "learn about" the members of a stimulus dimension for which he does not have a set or hypothesis. The *second*, not previously explicit in noncontinuity writing, stated that the presence of one set may preclude the establishment of a second set about another stimulus dimension. The *third* "postulate" established Lashley's affinity with Haire and the changes in the noncontinuity theory. Thus, while systematic responding might be taken as indicative of the existence of a set or hypothesis, Lashley asserted that the absence of systematic responding does not mean that a set is not present. Furthermore, the experimental designs and their interpretations employed by Lashley clearly indicate that the presence of a set or perceptual organization is not necessarily indicated by the presence of systematic responding.

It seems likely that criticism concerning the seeming indeterminacy of this formulation would have been presented immediately had Lashley applied it in interpreting the evidence from the reversal-design experiments. But Lashley did not follow Haire in this way. On the basis of his analysis, discussed above, of Spence's views on "seeing" the relev-

ant stimuli, Lashley was convinced that the reversal paradigm was no longer appropriate. A new type of experiment was offered, in which it was proposed first to establish in the organism a strong original set to the members of one stimulus dimension, then to insert members of another dimension into the situation while the first dimension was still present and "relevant," and finally to test for "learning about" the second dimension. Lashley asserted that continuity theory would predict that such learning would occur, whereas his theory would predict "that no new association will be formed, provided that the new component does not arouse a perceptual organization dominant over the first" (Lashley, 1942, p. 258).

Consider the following experiment. The S is trained to some criterion to choose one member of some stimulus dimension over a second member of that dimension, *e.g.*, he comes to choose a large circle over a small circle. Then in the second phase a large *triangle* is presented with a small circle. "Largeness" is still the positive cue, and the S continues to respond consistently with respect to it. What can we say about the S's "hypotheses" or "sets" in this second phase? By the response-definition criterion, the S has two hypotheses, size and form, since he is responding systematically to both large-small and to triangle-circle. Thus the *classical* noncontinuity prediction would have to be that the S had two hypotheses and thus was learning about both dimensions, and that he would therefore demonstrate systematic responses to form cues in the test phase. This prediction is apparently also that which the continuity schema would make, assuming the stimulation requirement is met, and is opposed to Lashley's prediction.

To fully understand Lashley's prediction, we must consider the qualification, ". . . provided that the new component does not arouse a perceptual organization dominant over the first." The difficulty, of course, with this position is that the key term 'set' and 'perceptual organization,' are left undefined. The solution, from the viewpoint of non-continuity theory, lies in the eventual definition of the critical concepts. It would be consistent with Lashley's style of conceptualization if these definitions were in physiological terms. We shall return to this viewpoint later. Our purpose here is to underline the methodological quicksand beneath the apparent power of the phenomenological words in the "new" noncontinuity theories.

Some Recent Developments

In 1955 two articles appeared in which the authors interpreted their data as evidence against the continuity position (Lawrence and Mason, 1955; Goodwin and Lawrence, 1955). The basic experimental design of these studies was as follows. Animals received several discrimination problems in succession in which hurdle-height and brightness were alternately the relevant dimensions. Both differential stimulus dimensions were present in each problem. For the first discrimination problem reward was correlated with a brightness cue, black or white.

When this discrimination had been learned a second problem was presented in which black and white were still present but in which the reward was now correlated with one of the hurdle cues, high or low. This training was followed by a third discrimination problem in which, for one group (the CD group), brightness was again relevant with the same brightness cue positive as in the first discrimination, while for the second group (the CDR group) the brightness cue that had formerly been positive was now negative and *vice versa*. The hurdle problems followed the same pattern. Thus on every second problem the CD group always relearned the same problem, while the CDR group had the reward value of the cues reversed. The two members of the irrelevant dimension in each problem each were correlated with reinforcement on 50% of the trials.

Lawrence and Mason stated that continuity theory would expect that, because of the reinforcement contingencies present, the associative strength of the irrelevant cues in each problem should become equalized. It should then make no difference in the next problem whether the reward values of these cues were the same or different relative to the previous problem in which these cues were relevant. Thus the two groups, CD and CDR, should not differ. On the other hand, Lawrence and Mason argued from their own "noncontinuity" position that the reversed group (CDR) should be retarded because the irrelevant cues in each problem are nonfunctional. Consistent with the latter position, which we shall look at presently, the results indicated that the CDR groups in both experiments took significantly longer to master the discriminations.

With respect to the continuity position, it is doubtful whether in such a complex situation as we are considering continuity theory can provide a specific prediction. From this point of view, the problem would probably most profitably be treated as an empirical one for further analysis. Note that during the second discrimination problem the reinforcement schedule with respect to the previously relevant dimension is a complex one. The percentage reinforcement of responses to the formerly positive cue is initially 50%, but increases to 100% as Ss come to respond to this cue only when it is paired with the new positive stimulus. Responses to the formerly negative cue may seldom receive nonreinforcement, since they occur initially with a low frequency and eventually occur only when the new positive stimulus is presented. The crucial point in the interpretation of the results is thus concerned with the effects of this training on the previously discriminated cues while the next problem is being learned. It would be desirable to show in some direct fashion whether the response strengths elicited by these cues do in fact become equalized under the reinforcement conditions present during the second phase of the Lawrence experiments. It is conceivable that these tendencies do not become equalized readily under the reinforcement schedules present. If this

were the case with, say, the difference remaining even without the presence of a new stimulus dimension, we would have an interesting result to be explained. But at the same time such a result would suggest that a noncontinuity analysis of the Lawrence results may be unnecessary. Such an experimental approach might utilize running speed measures. On the assumption that speeds are directly related to the effective excitatory strengths of the various cues (Spence, 1956) one could directly assess cue strengths during each phase.

An alternative interpretation offered by Goodwin and Lawrence (1955) was characterized by these authors as a noncontinuity position. Basically, it involved two simultaneous learning processes, one of learning to orient so as to receive stimulation, and the second of learning to respond differentially to the relevant cues once the first process has taken place. If both discrimination problems involve such dual learning and if it is assumed that the two kinds of learning involve different rates of acquisition and extinction, the formulation ". . . allows the preference for one stimulus aspect to retain its habit strength and remain nonfunctional even though it is present in the physical environment while the Ss are systematically reacting to other stimulus aspects" (Goodwin and Lawrence, 1955, p. 442). Thus when a new problem is presented, *e.g.*, hurdles after a brightness problem has just been learned, the responses of orienting toward the formerly relevant cues may extinguish faster than the responses of choosing differentially between them. And when the cues are no longer being received response tendency equalization cannot take place. The formerly relevant cues are "functionally absent" and thus remain unaffected by reinforcement and non-reinforcement.

From our discussion of continuity and noncontinuity theories, it seems probable that to this point the explanation offered by Goodwin and Lawrence is an extension of the "receptor-orienting acts" notion which Spence (1940, 1945, 1951) and others (Wycoff, 1952) have elaborated. Goodwin and Lawrence realized, however, that their own experimental situation did not seem to require special overt orienting behavior, and the formulation outlined above was modified to become what can be recognized as very similar to the later type of noncontinuity position. Thus they pointed out that although their situation did not require overt orienting responses, analogous "identifying" responses may take their place in the explanation. If the assumption is made that these responses are learned and extinguished faster than the differential choice responses, the results of their experiment would be explained. It would appear that this is a shift to something quite like "attention" and "set," and that what finally emerges is a noncontinuity theory similar to Lashley's (1942). It is distinguished by an attempt to relate the perceptual processes to the process of learning.

Goodwin and Lawrence made no distinction between receptor-

orienting behavior and "central" identifying responses, wrongly classifying both as noncontinuity mechanisms. To the present authors such a distinction seems to characterize the core of the post-Lashley debate. Thus we might formulate the basic features of the latter-day continuity and noncontinuity formulations as follows:

Continuity: If discriminably-different cues are received at the receptors, differential associative strength to these cues will develop. (We assume an objective definition of 'discriminably different,' such as the schema presented in an earlier part of the paper.) *Noncontinuity:* The presence of discriminably different cues on the receptor surface is no assurance that differential associative strengths to these cues will develop. Certain additional factors serve to limit the effectiveness of these cues. A statement parallel to the continuity position could be formulated as follows: If discriminably different cues are received at the *X* level of the nervous system (central with reference to the receptor) differential associative strengths to these cues will be developed. Important variables intervene between the retina and the *X* level. (We assume that a definition of 'discriminably different' could be formulated which would be structurally similar to the one above.)

Methodologically, the noncontinuity position offers several frustrating features. The *X* level has proved difficult to identify. The "certain factors" mentioned seldom are specified in any more adequate manner than by naming them with terms which have face validity with reference to the scientist's phenomenological world but no clear reference to that "other person" which we believe the rules of the game require we study. It may be noted that recent physiological experiments (e.g., Hernández-Péón, Scherrer, and Jouvet, 1956) would appear to offer encouragement to the general noncontinuity position by suggesting research directions toward eventual physiological specification of noncontinuity variables. And it should not be necessary to emphasize that the relative methodological advantage of the continuity position does not guarantee its empirical adequacy.

Summary

In summary the following points can be made:

1. The original or "classical" positions in the continuity-noncontinuity controversy were concerned only with the question of whether an animal "learns about" environmental events to which he is being differentially reinforced but is not systematically responding differentially.

2. Later elaborations of both the continuity and noncontinuity positions presented certain problematical features, the most important of which have been (a) Spence's requirement that *S* receive "discriminably different proximal stimulation" and (b) the noncontinuity requirement that *S* have the appropriate "set," "hypothesis," or "perceptual organization," if learning is to take place.
3. The phrase "discriminably different proximal stimulation," which at times has been misinterpreted as an undefined "perceptual" concept, is best interpreted to mean stimuli to which the organism can learn to respond differentially.
4. When the noncontinuity theorists abandoned the systematic-response definition of "hypothesis" and failed to provide a substitute definition for this term which retained a central position in the theory, a fundamental indeterminacy resulted. The one unequivocal statement remaining was that even when Spence's stimulation requirements were met learning may not occur.
5. The designs of two recent experiments of Lawrence and his co-workers do not permit a specific prediction from continuity theory, and permit only a quasi-prediction from noncontinuity theory because the perceptual vocabulary applied is still lacking in objective definition. Under these conditions the data can not be considered crucial to the existing controversy.
6. The essence of any controversy that may exist today in light of the reservations mentioned above is contained in two alternative hypotheses as to the sufficient locus of discriminably-different sensory input events in behavior theory: (a) the receptor surface, and (b) some level central to the surface, there being certain factors "in between" which make the receptor surface a necessary but not sufficient locus.

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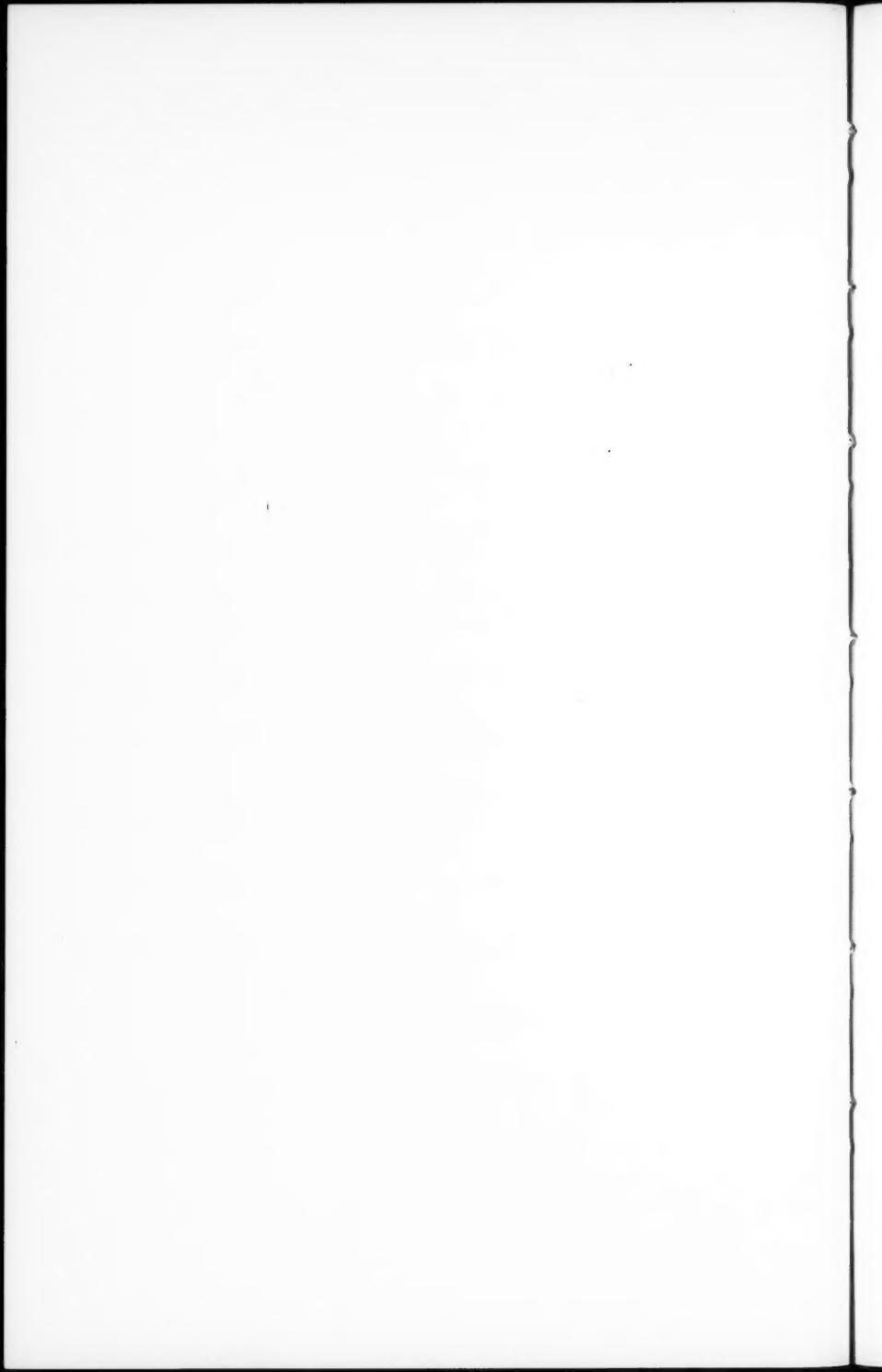
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SOME ASPECTS OF VALUE IN PSYCHOLOGY AND PSYCHIATRY

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The problem of value, that is to say, the circumstances under which a value arises for a human subject, is elaborated through increasing experience and intellectual attention, and is said to be *expressed* by behavior when the subject faces a given problem in a specified social context, is increasingly being given attention in the behavioral and social sciences. In their concern with problems of value scholars and working scientists are beginning to discover that, at *least*, the initial portion of the problem is often a semantic and a logical one. They are also beginning to discover, as a result of this fact, the rich plurality of meanings which have been attached to this concept. The reader can easily convince himself of this by consulting Albert and Kluckhohn's (1960) recent bibliography which lists for the period of 1935-58, alone, over 2000 references to research on values. The variety of competitive notions concerning what is meant by value, is overwhelming. At least one scholar, Pepper (1949), has openly declared this situation not only to be welcome but probably also necessary and fruitful. Amidst the welter of conflicting approaches the notions with which psychologists have chosen to work represent a rather limited selection of the many conceptual aspects to the problem of value,—a problem which seems to be central both to behavioral theory, psychotherapy and policy-making in the social sciences.

The problem of value takes on many forms in the behavioral and social sciences. The sociologist, George A. Lundberg (1948) has stressed the fact that the proper enterprise of science is to restrict itself to all inquiries which can be talked about in the form of "if-then" statements. At the same time he, himself, has emphasized in his teaching that problems of value adulterate the scientific enterprise in many ways. The scientist's endeavors may get tangled up with considerations of value in his choice of a field or problem, in his choice between two competing but equally efficient hypotheses, in his decisions as to what data are relevant to a problem, in his choice of methods for collecting such data, methods of analyzing them and methods of drawing conclusions from them, in determining confidence levels which justify conclusions, in deciding what applications are to be made from his findings, in harmonizing these findings with the norms of his culture and in deciding what redesigns of his culture are implied by his scientific conclusions. The degree to which values may act as the mainsprings of scientific endeavor has been brilliantly and systematically elaborated upon by Polanyi (1958), without, in my estimation, declaring that such sources of moti-

vation for inquiry are inevitable. On the other hand Myrdal (1958) as a social scientist has not only declared for years that *values cannot be* separated from scientific inquiry in many of the behavioral (e.g. social psychology) and social (e.g. economics) sciences but even more that the social scientist should make explicit the values which have inescapably directed his research.

Psychologists of every vintage—from representatives of extreme radical behaviorism to extreme mentalism via some of its current, existentialist varieties—sooner or later pay their respects to this problem. Most treatments of the problem are, of course, “mentalistic” in nature but if this term is repugnant to the reader, let us say that most approaches are cognitive, phenomenological or introspective in nature. This is necessarily so for throughout its entire history axiology has borne one common meaning for all notions of value, namely, that however differently the concept may be defined by writers, in most cases the “holding of a value” refers to a state of mind or consciousness. Nevertheless even anti-mentalists are forced to make passing reference to the concept although, as is to be expected, this is done by so redefining value that all its mental content is naturally squeezed out of it. Whether any gain will permanently result from this form of throwing out the baby with the bath, remains to be seen.

Consider Skinner, the champion of an anti-mentalistic, radical empiricism. In one of his better known volumes Skinner (1953) has discussed value in at least two senses, namely *adaptive values* and *exchange value*. In his discussion of the design of a culture he appears to be defending, or at least preferring, a position in which he gives this concept content solely in terms of the *survival value* of individual and group practices. This is, of course, the conventional meaning of the concept of *adaptive value*. To be fair to Skinner it must be said that this imputation is qualified considerably by the recognition of difficulties which surround the notion of survival value, for instance, such matters as the *changing* survival value of a given practice with the passing of time and the difficulty of *calculating* the survival value of a new practice which is proposed. Elsewhere in the same volume Skinner discusses *exchange value*. In his own words

... An object is “worth” to an individual just that amount of money which he will give up in exchange for it, or in exchange for which he will give it up. Before an exchange or a sale can occur, certain critical values must be reached or exceeded. A will give the article to B if the aversive consequences of this act are roughly matched by the positively reinforcing consequences of the money which B will give to A. B will give this amount of money which he will give up in exchange for it, or in exchange are matched by the positively reinforcing consequences of receiving the article from A. (p.394)

It is doubtful that either a microeconomics or a macroeconomics can

be built up systematically and theoretically from this typical, Skinnerian basis. In order to do Skinner justice, however, it must be pointed out that he states that the "reinforcing effect of either goods or money cannot be stated without taking into account many different characteristics of the *history* (italics mine) of the individual buyer or seller . . ." (p.398) and I strongly suspect that, at the level of complexity which governs economic transactions today, a bow to this history smuggles in by the back door some of the mental content which was released through the Skinnerian front window.

It is difficult to conceive of economic behavior, carried on as it is today through linear programming, econometric theory, theory of games, operations research, etc., being studied and explained without reference to the highly abstract intellectual processes which appear to mediate it. It is equally difficult to believe that the introduction of concepts such as the *mand*, *tact*, *autoclitic*, etc., introduced by Skinner (1957) in his "Verbal Behavior" will be sufficient to displace concepts such as value and *intentionality*, in accounting for complex behavior in general. We cannot, however, prejudge the issue. Skinner has certainly attempted a first approximation towards this displacement in Chapter 18, Logical and Scientific Behavior and Chapter 19, Thinking, in the volume in question. But to return to the passage quoted above. Inasmuch as Skinner has defined an operant as *a class of responses*, there would also appear to be some difficulty involved in predicting the probability of a buyer-seller exchange of economic goods purchased or sold *for the first time*—a Rolls Royce, a painting by Rubens, the manuscript of a first novel, etc. In general the problem of value (not just economic value) has been treated rather skimpily by Skinner. One looks in vain, for instance, for an extended treatment of it in the volume just mentioned, where one would expect to find it. But perhaps this only indicates that no discussion of it is feasible, except in terms of consciousness.

We have dwelt on Skinner's views because in a sense Skinner regards a term like "value" as an explanatory fiction which has to be exorcised from science. It is not that his views are so essential to a discussion of the problem of value. It is rather that they suggest the point already emphasized, namely, that this problem of necessity must take consciousness as its epicenter.

The present paper is clearly not the place to review those notions concerning the meaning of value which do assume consciousness as its source. A brief outline of some of the major classes into which values naturally fall, has been given by Wheelwright (1954), namely, values of possession, values of action, values of contemplation and values of existence, with some of the sub-divisions which appropriately belong under these categories. Perry (1954) has written a definitive classic dealing with the manner in which the concept of value receives expression in all fields, ranging from social organization, social institutions, the cultural sciences and their methods, ethics, politics, law and jurisprudence,

economics and modern science to art, history, education, metaphysics and religion. Pepper (1958) has produced a magnum opus on value, which will be of maximum appeal to psychologists because its approach rests completely upon psychological theory and experimentation. To these and other sources the reader should properly be referred. There are two aspects of value, however, of relevance to psychological theory which I should prefer to take up here. One of these is concerned with the relation of value to scaling theory, the other with the relation of value to psychotherapy. I should like to point up some considerations usually overlooked, I believe, which I think justify the assertion that problems of value are germane to the conduct of certain lines of research.

VALUE IN RELATION TO SCALING PROCEDURE

One of the major current concerns of psychologists is the *measurement* of value. This is generally undertaken through the application of a variety of mathematical and quantitative techniques. These are concerned by and large with direct studies of individual choice. This may be clearly discerned in such a paper as that by Adams and Fagot (1959) which deals with choices that carry no risk and which are made by individuals from among pairs of alternatives each of which is, itself, available in a two-choice situation. It may also clearly be seen in such a contribution as that of Suppes and Walsh (1959) in which choices bearing risks have to be made from two or more situations and where such a choice depends upon the individual's best guess as to the probable consequences of his actions and the *value weight* he attaches to each of these consequences. The assessment of consequences is said to be governed by the chooser's "subjective probabilities" while the value weights are said to represent his "utility function." Considerations such as these also enter into our own concern with the concept of "adjustment," discussed in the second section of this paper, where they are alleged to play a covert role in some of the practices of modern psychotherapy. We are, however, less concerned with situations in which it is explicitly recognized that values arise than those in which there is a strong tendency to overlook the fact that they are, in part, present and must be appealed to for a proper treatment of the context in question.

One area in which problems of value are covert is scaling theory. The sense in which both methodological and philosophical problems of value are implicit in the applications of scaling procedures is generally, however, of little direct concern to the psychologist who is busy with the construction of scales and their social meaning. Consider, for instance, the construction of a scale by the method of paired comparisons, aimed at determining the degree to which each of a set of statements is seen as favorable to some psychological object. The first point to be made in the present context is that in some sense of the term "value," each subject is consciously or unconsciously expressing one or more

values each time he makes a paired comparison. The value criterion from whose vantage point a preference is expressed every time a subject makes a paired comparison has, of course, no direct bearing on the legitimacy of the scaling procedure, itself. The variation in value criteria from one paired comparison to the next is part of what we mean by the operation of chance factors which underly the process of discriminable dispersion. Nevertheless there is some merit in undertaking the task of making explicit the manner in which such value considerations play their role. Consider the nature of the context involved. Statement A may be judged as more favorable than statement B, with respect to the psychological object, X, because of some criterion of value, V_1 . Statement B may be judged as more favorable than statement C, with respect to the same object but this time because of another criterion of value, V_2 . Thus the criteria of value may vary within the same individual from one paired comparison to the next and from one individual to another for the same paired comparison. The *value context* involved is, as we have already said, ignored in the establishment of such a scale. One consequence of this is the difficulty of determining the reliability (stability) of the value judgments involved, over time. If subjects were asked to make paired comparisons for the same context on two different occasions, the value criteria invoked by the same subject making the same paired comparison might vary. Even if we could get a subject to make these criteria explicit and found that they varied over two different occasions, we would be unable to distinguish between this variability as an expression of uncertain or competitive values over time and the same variability as an expression of a *change in values*.

Let me illustrate briefly what I mean by a criterion of value in the present context. The following two statements are taken from Edwards (1957) in discussing a study which Hill did during the Korean War. A. Winning the Korean war is absolutely necessary whatever the cost. B. We are protecting the United States by fighting in Korea. Subjects were asked to compare them according to the degree of their favorableness relating to the participation of the United States in the Korean war. This last, of course, is the psychological object. Subject X may declare A > B because he judges that A recognizes more clearly than B that freedom is at stake and that no price is too great to pay for it. This represents an unspoken value. *One way* of expressing this unspoken value might be as follows: V_1 . Social and political freedom should be preserved and fought for, no matter what the odds and the cost may be. In the light of V_1 , A is declared to be *more favorable* than B to U. S. participation in the Korean War. Subject Y may judge B > A for quite another reason. In his judgment the unspoken value that should be invoked might be stated as follows: V_2 . In war national self-preservation is the basic consideration and appeals to abstract justifications, like the preservation of freedom, are irrelevant. These two values, V_1 and V_2 , are not, of course, the only pair of value criteria which subjects may invoke in making their paired comparison judgments. They merely

represent a sample pair of possibilities. Clearly, however, they are not identical. It is also not to be forgotten that either X or Y alone could invoke each of them for the same paired comparison presented on two separate occasions.

The preceding remarks, of course, represent the ideal value context within which a mature subject would make a paired comparison judgment. This ideal context would be one in which the value criteria invoked were *perfectly explicit* for the subject, himself. Unfortunately there must be an overwhelming number of occasions on which value criteria play a role but do so as vague reference points of which the subject is not fully aware. In such instances the judgment, A is more favorable than B towards the psychological object in question, amounts to an elliptical formulation in which the value criterion involved is unexpressed by the subject to himself and is therefore both unclear and uncertain. This would, of course, be avoided if, when using scaling procedure, we required a subject to verbalize his judgment via an assertion of the form "A > B for object, X, because it *fulfills* (expresses) V_1 more readily." It is to be noted that such an assertion need not commit the subject, himself, to V_1 although, in fact, the criterion invoked on most occasions probably also reflects a value held by the subject. A subject can, of course, invoke V_1 without holding it. All he need be concerned with, consciously or unconsciously, is a value basis for making the comparison.

The elliptical formulations of which I speak allow a pluralistic and often contradictory context of values to underly the scaling operation. It is probably a consideration of this sort that gives the *zeta function* its utility. An inconsistent circular triad of the form $A > B > C > A$ may often be due to the fact that the common value criterion invoked for the paired comparisons, A-B and B-C, may not be the same one which is invoked for the paired comparison, A-C. By the same token, u , the coefficient of agreement, may be equal to 1 for two judges and yet this may be achieved through the invocation of different value criteria underlying the paired comparisons of both judges.

All of the preceding value considerations tend to be obscured by the ideas underlying the process of discriminant dispersion to which, of course, they are entirely irrelevant from the standpoint of scaling procedure. Nevertheless the single case in behavior, even if we are only concerned with the individual's position on an attitude scale, may be understood better and more completely through some knowledge of his values and their organization than by studying their manifestations through expressed attitudes. These attitudes stand to the organization of values as symptoms do to causes.

In effect, then, the establishment of a unidimensional scale disguises the fact that intersubjective and intrasubjective choices reflected in the paired comparison judgments obtained, proceed from a value-manifold. If subjects were forced to make the value criteria which underly these

paired comparisons explicit, this would soon become apparent. The unidimensionality assumed in scale analysis rests on the *affect* induced by attitude statements. It is not, however, concerned with the cognitive value criterion with which this affect is associated. If the value criteria of subjects were made explicit and if these were subject to a factor analysis, it would become clear, I think, that the universe of values underlying paired comparison choices was multi-dimensional and that this is perfectly consistent with the measurable existence of a unidimensional affect as an accompaniment of a multi-dimensional value space. In fact, this is precisely what Morris (1956) obtained for a factor analysis of the 13 Ways of Life he employed in his studies of value. These yielded several dimensions of value which underlay the value criteria which he explicitly furnished his subjects. To the extent that a scaling procedure provides for a continuum of *affect only*, to the extent that the quantitative rationale which underlies the assumptions made for the process of discriminant dispersion and the deductions drawn from these assumptions likewise center about the ordering of affect, to this extent the more central questions of value which underly our choices are unnecessarily ignored. In ignoring them we may be missing more than we are bargaining for. If the economist, Boulding (1956), is correct in his eiconical notion that most of our significant behavior is determined by our images, particularly our value-images, then research dedicated to understanding the genesis, maintenance, expression and alteration of these images may offer both more predictability and control as well as more understanding (in the *verstehende* sense of the word) than any effort on our part to determine the median *affect position* of a subject's attitudes from the use of a scale. This affect is extremely important but it does not tell us anything about the subject's value-images through which he positioned himself on the scale in question. A knowledge of these value-images would certainly be much more revealing but a knowledge of these, together with their accompanying affects (provided we assume the commensurability of these affects) would furnish an even stronger basis for dealing with behavior in social contexts.

THE ROLE OF VALUES IN PSYCHIATRY

In the present section I should like to examine the role which questions of value play in the field of psychiatry for here problems of value are more sharply commingled with psychological considerations than they are in the fields of scaling theory and attitude research. Thomas S. Szasz (1960), in a recent article, has emphasized the increasing extent to which the modern psychiatrist is becoming a "social tranquilizer." By this Szasz refers to the tendency of the typical psychiatrist to see almost all psychotherapy in terms of the patient's "adjustment." As Szasz sees it, the typical psychiatrist wishes to protect "the harmony of existing (chronic) institutions, such as marriage, social class, profession (as guild), nation, etc. Faced with conflicting values and social aspirations, psychiatrists may now interfere in order to obscure and evade the issues. Relief is offered by focusing the con-

flicting parties' attention on a substitute problem and its possible solution" (p. 561). Much of this of course, occurs in the name of "adjustment." Psychiatry, according to Szasz, as a conservative, institutional force is a relatively new thing and is inimical to the values of science and democracy. It obstructs needed changes, obscures social problems, prevents needed modifications and abandonments of parts of existing value patterns and acts to reinforce conformity to the status quo, regardless of consequences. This is the type of allegation which is in line, of course, with Lindner's (1952, 1956) strictures concerning the psychotherapists' obsession with the notion of "adjustment" which, Lindner insists, has become a sacred cow. There is an expanding literature today on the abuse of this notion, a notion which underlies the psychiatric task of social tranquilization. What is intriguing, however, from a professional standpoint, is the relatively superficial treatment which the notion of adjustment or social adaptation receives in the existing literature. I should here like to examine this concept somewhat more closely, since it is so intimately bound up with such concepts as mental health and personality integration and so closely related to those techniques of psychotherapy through which the patient is supposed to be helped.

The manner in which we speak of adjustment is always in terms of an existing *problem situation* for the individual. We customarily recognize that there is a state of affairs to be brought into being for him or a state of affairs to be dissipated. This state of affairs can, of course, be concerned with the internal psychological tensions of the subject, his faltering self-image or any other shaky phenomenal framework. It need not involve only a readjustment to some aspects of the not-self. When the required changes have been accomplished we say an adjustment has been achieved. I should like to suggest that the *psychic side* of the adjustive process involves a question of value, that is, I am suggesting that a problem is solved (an adjustment or adaptation is made) in order to achieve a value defined, as Boulding would put it, always as an *image of some state of affairs* we desire to achieve—status, the love of another, the full expression of our talents, a self-image which is also acceptable to others—together with a train of images of prospective behavior which is judged capable of bringing these states about in specified social and physical contexts. In short it is truer to say that our actions are concerned with the modifications of our milieus only insofar as these modifications enable us to realize one or more of our values. I realize that this is precisely the stance eschewed, for instance, by Skinner who would prefer to deal with our behavior in terms of positive reinforcement and aversive conditioning, not because of any denial of the existence of mental events but rather because they do not permit us to operate with any manipulable variables. However, I am convinced that in order to give any communicative content to the concept of value, an appropriate departure must be traditional and stress inner events, cognitive processes and phenomenal frames of reference.

Granted the necessity for talking about values within a context of consciousness (for images are always psychological states), there are nevertheless other difficulties even though we are accepting without question a framework which involves a mentalistic bias. Some values are substantively adjustive and some are only mediately adjustive, that is to say, some values are intrinsic (good, in themselves) and others are only instrumental—steps on the route to the achievement of such intrinsic values. In striving to help a patient adjust, a central consideration should certainly be one which recognizes the difference between the patient's substantive and mediating values. At the same time any directive therapy which recommends given moves or changes in the patient's life-situation should certainly not violate the spirit and feeling-tone of the patient's substantive values. Further than this the therapist must keep his own value pattern out of the picture. It is the patient's substantive and mediate hierarchy of values which is involved in the therapeutic situation. This is the justification for the existentialist stress on entering the *Eigenwelt* of the subject. Obviously there are really two types of *directive therapy*, one which proceeds from the phenomenal frame of reference of the therapist and one which calls for counsel from the more or less successful adoption by the therapist of that of the subject. Szasz is clearly complaining of therapy cast in the former mold—particularly where, self-consciously or not, the psychiatrist is acting as an uncritical proxy for his culture. A knotty problem arises when we ask under what circumstances the psychiatrist's frame of reference should be allowed to interpenetrate and displace that of the subject and remodel it nearer to the psychiatrist's heart's desire. This is a value problem with a vengeance. The usual answer is to invoke a value metatheory, that is to say, a procedural rule, *itself a value*, to which patient and therapist can give common consensus and by which the weight of their respective but competing value-systems can be hierarchically ordered.

When enacting uncritically his function of social tranquilization the psychiatrist is accused of encouraging the patient to adjust to a given pattern of values. Where the psychiatrist acts as proxy for his own culture, he is in effect, if we may be permitted once again to lapse into existentialist terminology, asking the patient to adopt what might be called the *modal Eigenwelt* of that culture. This is certainly equivalent to asking the patient to adopt a particular pattern of mass values—or any part of that pattern. To do this, however, raises again what I have called a metatheoretical issue. By what higher-order criteria do we judge that one value should be pursued rather than another? If, as Skinner has emphasized, that criterion should inhere in the survival value for the individual or the culture of the behavior which expresses the value(s) in question, there are *at least* two difficulties involved. One of these Skinner has noted himself. That is the difficulty of estimating the *prospective* survival value of the activities (adjustments) which the *intentionality* of the values we hold, dic-

tates. This estimate has to be made for a future time under contingencies which will not be within our control. The consequences of these adjustments, both immediate and remote, have to be guessed at. Furthermore these consequences have to be weighted for the degree to which they can be expected to facilitate or inhibit the expression of values which are currently part of our total value pattern. It should be possible *in principle* to develop a technique for *operationally* assessing the probable total sum of *value satisfaction* which the train of consequences set in motion by our adjustments, will achieve over any stated period of time. The metric thus devised would have to combine the best features of an accounting estimate and operations research methods, since the consequences involved would have both positive and negative weights while a *value accounting period* would certainly have to be designated for a sort of *axiological, inventory control*. Needless to say no such metric exists and therefore we have at present only the vaguest ideas of how to *estimate* the survival value of an individual's or a society's adjustments.

The second difficulty is that when an individual chooses to make an adjustment in order to achieve a given value (and the psychic satisfaction which accompanies that achievement), he, alone, is in a position to declare what value he seeks and whether the expected satisfaction has, in fact, been derived. Hitler's basic value underlying some of the alleged adjustments he made to lessen the difficulties of the German people, was to try to achieve world hegemony for the Herrenvolk (and thereby, himself, of course) and the status and emotional satisfaction which these would presumably bring in their wake. The means were unimportant—war, deceit, torture, *schrecklichkeit* in every form. But these means were, of course, the forms of adjustment Hitler chose in order to achieve his value. We naturally recoil in horror from the *consequences* of these adjustive procedures as well as from the use of such procedures, themselves. Had Hitler succeeded, however, only he could have declared that they had conferred the value-satisfaction which he had originally sought.

There is an undefined moral tone to human striving, however, which makes us recognize that there must be some complementarity between means and ends. We are horrified at the implicit principle involved in Hitler's actions, namely, what might be regarded as the analogue to the Goethe—Carlyle—Croce—Spingarn aesthetic credo. That credo asserted that if an artist succeeded in doing whatever he set out to do, with whatever materials he had at hand, the item produced was a work of art. By analogy if a madman succeeds in achieving satisfaction by choosing to implement a value by whatever means he can get away with, then the value is well chosen, the means must not be looked at too closely and the consequences of the madman's actions are not to be taken stock of. Put this way we immediately reject this *folie de valeur*. In the example discussed our repugnance centers chiefly about Hitler's mediating values rather than his goal-value of world

hegemony, since this latter is and has been a commonly accepted goal-value of Western nation-states for a long time. But the relation between a patient's goal-value and the *metacritical justification* of that goal-value is exactly the same as that between mediating and goal-values, themselves, as in the example given. A sense of complementarity which has to be explicitly defined, is involved in both cases. Nevertheless, and this is the point, this is precisely the process which is involved when the psychiatrist encourages the adoption of a value—whether a going one culturally or not—namely, that he is not asking himself what metacriterion is involved in advising a patient to adjust to one given value rather than another. The value he sells the patient produces a train of consequences which may or may not be calculable and is, itself, not rationalized by any further criterion. In addition the value chosen also places no restrictions on the means by which the patient may try to realize it.

In recommending *in principle* that a patient achieve a given value, that is, make a given adjustment, there are, in fact, *implicit metrics* involved other than the one to which we have made reference in preceding paragraphs. Axiology is a metrical domain in many senses. One of these senses must be invoked in the present context. The psychiatrist does not really recommend an *unqualified* adjustment. Because his advice is elliptically formulated he appears to be asserting ideally that the subject (the patient) should make response R (or a train of specified responses), to situation S, provided that R produces consequence C (or a set of specified consequences). I say ideally because the form of the assertion which I have just made refers presumably to a highly sophisticated psychiatrist who tries to foresee some of the *immediate* and *remote* consequences of the specific actions he encourages in order to help the patient achieve given values and their attendant satisfactions. Even this, however, would be insufficient. Actually our prototype psychiatrist wants C to be achieved in the shortest possible time, with the smallest possible cost in money and expenditure of energy, with a minimum of resistance from an interpersonal context, with a declared maximum satisfaction with the new behavior which is being inaugurated, on the part of those affected by it in the interpersonal milieu in which the patient behaves, etc. In short a budget of consequences following from the behavior which is to be instituted, is recognized, and the desirability or undesirability of these consequences, themselves, is recognized (or could be) as constraining the quality of the adjustive behavior recommended. But in addition attributes like time, cost, energy expenditure, social resistance, social satisfaction, etc., are capable of being so defined operationally as to be measureable and, what is even more important, they exist on a continuum. Therefore alternative courses of adjustive behavior recommended can, *in principle*, produce variable values of these attributes of the behavioral sequelae, so that the desired maxima and minima can presumably be shown to be creditable to certain alternatives and not others. If one given

alternative produced all the required maxima and minima, presumably this would be the most appropriate form of adjustment, assuming, of course, the acceptability of the set of immediate and remote consequences which resulted from this alternative. This, however, is not likely to be the case. As a result one requires a metric which will provide a means for choosing an alternative from a set each member of which produces at best only some of the maxima and minima and, in many cases, none of them. The formulation of this *secondary metric* (as contrasted with the *primary metric* used for determining the *desirability or undesirability* of different members of the set of consequences which flow from our adjustive choices) requires a high degree of mathematical sophistication and is obviously never involved in the psychiatrist's recommendation of one course of adjustive action as opposed to another.

There are still other implicit metrics in the psychiatric doctrine of adjustment. If values are the psychic and existential side of motor-affective adjustments, then those types of adjustment which help to inaugurate psychological states of conflict are not adjustments at all. A recommended move may achieve a given value but also bring into being consequences which are the expression of disvalued states of affairs (Skinner would say, aversive consequences). Thus any recommended adjustive move may bring into being consequences which support both values and anti-values. If, because of contingencies over which neither psychiatrist nor patient have any control, all alternative forms of adjustment which can be recommended will create consequences that fulfill both values and anti-values, then we require a hierarchical set of positive and negative weights for these values and disvalues. These weights are not to be confused with a metric for the consequences themselves. The latter metric involves differential weights, both positive and negative, which furnish a scale number that expresses the degree to which each such consequence fulfills a given value, say V_1 . The former metric involves, at the very least, an ordinal prior mapping of our finite set of values, themselves, before we institute any adjustive behavior. In short it establishes hierarchical order among our values and hierarchical order among the values we reject. If every alternative recommended course of adjustive action is expected to bring into being consequences which fulfill both values and anti-values, then both these metrics would come into joint functional play in some sense. As a result we would need in addition a calculus for defining an *optimum pattern or set* of combined value-fulfillments and their weighted, associated consequences, each such set corresponding to one and only one alternative course of action.

Unfortunately the practicing psychiatrist has neither the time nor the inclination to adjust his psychotherapy to such quantitative methodological and philosophical constraints. As suffering human beings his patients require immediate and extended help, emotional sympathy and interest and some sign by which not only do they become aware

that the psychotherapist accepts them but, in addition, some alteration of their own mental content by which they can learn to accept themselves. Even though the meaning-content of the concept of adjustment cannot be given practical expression in the workaday activities of the psychiatrist, it is nevertheless true that the adjustive task of the psychotherapist is incomplete and unsatisfactory in the degree to which his judgment does not reflect considerations such as those we have mentioned.

The point then of all our preceding commentary on the concept of adjustment, is this. A recommended course of action constitutes an adjustment. An adjustment eliminates a problem by fulfilling a value for the subject who has heretofore been blocked in achieving it. This value is, however, either a cultural norm, a fiat value of the psychotherapist or an idiosyncratic value for the patient. In neither case has it been analytically justified and to justify it empirically in terms of a criterion of survival for the patient, would involve various types of quantitative procedure which not only are today no part of the arsenal of theory in psychotherapy but could hardly be put into practical execution in a busy practice (although some such type of procedure might be feasible if a psychotherapist dealt with only one or two patients per day). Nevertheless these considerations serve to show, I think, the extent to which the problem of psychotherapy—particularly as a technique of enabling the patient to adjust to the world around him—is deeply commingled with the problem of value. The concept of adjustment is inextricably intertwined with questions of value, which is, perhaps, as it should be.

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FLESCH FORMULAS APPLIED TO CURRENT READINGS¹

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Toynbee in his *Civilization on Trial* (as abridged by Somerville, 1947) points out that where the environmental demands on a people are too great there is no important cultural growth. Similarly, if the demands are too slight there is not enough incentive for growth. Only when the demands are optimal, or nearly so, do a people respond to the stimulating challenge. This same concept may be applied to students. A study by Allen (1952) shows that how much students learn is affected by the demands made upon them: the human interest and the level of difficulty of materials assigned to them. Rudolph Flesch has given us the techniques for testing these demands, and studies have shown his techniques to be reliable measures (Klare, 1952) even in the hands of "inexperienced analysts" (Hayes, 1950).

Earlier investigators applying Flesch's formulas to general texts and collections of readings in psychology have answered the questions of how difficult and how interesting or dull is the material assigned to beginning students. Ogdon (1954) studied eight general psychology texts, and Anderson (1956), in a continuation of Ogdon's work, evaluated five books of readings in psychology. Both of these investigators found that the works they examined were of "comparable difficulty," that they varied from "mildly interesting to dull," and that a "readable article was not necessarily an interesting one."

The present study is a repetition of that by Anderson applied to five more recently published collections of readings in psychology which are meant to be used along with the regular psychology text. As additional reading they should, therefore, be stimulating and interesting. Are they? If so, to what degree, i.e., what is the level of difficulty and how much human interest is there? Also, by comparing the results of this study with those of Anderson's study, the investigator has sought to determine whether there has been any improvement in the readability of general psychology readers in recent years.

Five readers were used as the subject matter. They were: Beards-

¹This study was made for a course in professional problems under Dr. R. S. Daniel, whom I wish to thank for his guidance and encouragement.

lee, et al., *Readings in Introductory Psychology*; Daniel, *Contemporary Readings in General Psychology*; Dulaney, et al., *Contributions to Modern Psychology: Selected Readings in General Psychology*; Hartley & Hartley, *Outside Readings in Psychology* (2nd ed.); and McGuigan, *Current Studies in Psychology*.

Twenty-five articles covering six subject fields were sampled from each text. Three 100-word passages were taken from each article. Where the article was short (two or three pages) every third paragraph was chosen; where it was long (over four pages) the first paragraph on every other page was chosen. Nowhere were introductory or summary paragraphs used.

A readability score and a human interest score were computed for each article and from these were derived the mean and the standard deviation for each text. Then the human interest and readability scores within each text were correlated to show the relationship between the two. Also the range within each text was determined.

In addition, the readability and the human interest scores in the various subject fields were obtained by combining the data across texts to arrive at the mean and the range within subject fields, and to make comparable the means for subject fields with Anderson's earlier computations.

The results are given in Tables 1 and 2. As in earlier studies the results in Table 1 show that the current publications are about equally hard to read. Mean readability scores vary from 27.34 to 43.11 but all fall within the difficult to read category (according to Flesch: 0-30, very difficult; 30-50, difficult). The range within a given text is again wide, although less so than in Anderson's study. However, there is greater variability. In Anderson's work the standard deviations varied only from 9 to 14; in the present study they range from 9.52 to 36.00. The McGuigan text, in which many of the articles were revised for readability and human interest has the lowest over-all variability; the Daniel text, the greatest. Selections in the five texts ranged from fairly easy, 71.57, to very, very difficult, 1.82. Nowhere was there a readability score as low, however, as the 0 score of one of Skinner's articles in the Anderson study.

Some of the most readable articles were written by the National Research Council and Science Service, Gates, Kohler, Boring, Munn, O'Brien, Estes and Straughan, Skinner, and Allport. The most difficult were by Hernandez, Murdock, and Mowrer, the latter article being the most difficult of all with a score of 1.82.

In general there was a slight improvement in the level of human interest over that in the Anderson study. His means for the human interest scores varied from 8 to 15 or "mildly interesting to dull." In the present study the means for the human interest scores are 11 to 14

TABLE 1
READABILITY AND INTEREST SCORES OF FIVE GENERAL READERS IN PSYCHOLOGY*

Text	Articles sampled	Readability		Interest		r		
		Mean	SD	Range	Mean			
McGuigan	25	43.11	9.52	59.09-26.09	26.68	12.13	53.31- 8.48	1.00
Beardslee	25	39.10	16.42	70.53- 1.82	22.80	12.29	55.73- 3.63	.40
Dulaney	25	38.15	16.85	57.97- 9.85	24.59	16.27	54.73- 1.21	.42
Hartley	25	37.11	11.29	58.78-16.17	25.65	10.39	48.66- 9.69	.37
Daniel	25	27.34	36.00	71.57-11.13	26.39	12.40	47.25- 2.51	.90

* The lower the score, the less readable and the less interesting.

TABLE 2
FLESCH READABILITY AND INTEREST SCORES OF SUBJECT FIELDS WITHIN PSYCHOLOGY*

Subject	Articles sampled	Readability Scores		Interest Scores		Anderson's mean	
		Mean	Range	Anderson's mean	Mean		
Developmental	25	40.13	64.29-17.63	46.00	30.46	55.73- 4.84	9.00
Learning	25	42.45	67.59-16.17	49.00	20.90	42.03- 1.21	11.00
Motivation	20	36.91	59.09- 1.82	52.00	25.56	48.66-14.54	18.00
Perception	25	41.86	71.57- 9.85	52.00	21.36	35.13- 3.63	16.00
Personality	20	39.99	50.93- 8.90	49.00	25.12	41.29- 3.63	15.00
Social	20	37.33	51.19-11.13	48.00	36.77	47.46-24.23	14.00

* The lower the score, the less readable and the less interesting.

points higher than were Anderson's, showing an increase in the level of human interest. There is greater variability within texts as shown by the higher SDs and the greater range which varies from 55.73, or very interesting, to 1.21, or very dull.

Among the most interesting writers were Allport, Frank, Levin, and Thigpen and Cleckley. The least interesting articles were by Sperry, Gerard, Sessel, and Sears; an article by Morse and Skinner with a score of 1.21 was the least interesting of the total sampled.

An article may be readable but not necessarily interesting as shown by the correlation results. In three instances the correlation between readability and human interest remains close to that of Anderson's. His highest r was .37; whereas in this study the McGuigan text has an r of 1.00 and the Daniel text has an r of .90, showing that in those texts at least certain selections were both readable and interesting.

Some writers were relatively high in both readability and human interest: Levin, Allport, Munn, National Research Council and Science Services, and Asch. Others were consistently low in both scores: Piaget, Freeman, Mowrer, Morse and Skinner, and Hernandez. Still others were high in readability and low in human interest or vice versa: O'Brien, Murdock, Gerard, Sears, Skinner, and Biedler.

In Table 2 are given the scores for the within subject fields. The mean scores for readability are approximately 10 points lower than those of Anderson's study, but the mean scores for human interest are again higher (10-18 points). The widest range of readability is in readings on motivation and perception, or from fairly easy (71.57) to extremely difficult (1.82). Human interest scores vary the most in readings on learning and development, or from very interesting (55.73) to very dull (1.21).

The evidence indicates that there has been some improvement in psychology readers. However, the improvements have been neither uniform nor impressive. Human interest has been increased somewhat, but most of the selections remain difficult and fairly dull. According to Flesch, readability scores of 0 to 30 indicate the collegiate level; those from 30 to 50, some college training. Scores up to 50 in readability and up to 20 in human interest point to a style that is difficult and that is typical of the academic, scholarly, scientific, and professional magazines. Most beginning students would find the majority of the selections very difficult indeed. They would, in addition, find some of them rather dull and uninspiring. They do not have the background and have not yet had sufficient training to handle without much struggle material of this type.

Although there has been some improvement in human interest—more so than in readability—more could be done. Flesch has shown that writing can be improved in both areas, and has given us the

techniques for such improvement. The classic readings in psychology with which every psychology major should be familiar would probably have to be included as they had been written, but current writings could be selected or revised according to Flesch's suggestions. Writing to be scientific need not necessarily be difficult and dull as is shown by a number of selections in these readers. Not every article need be easy to read, but if we are to present our college students, on the beginning level, with the optimal challenge, we need supplementary readings which are moderately difficult but rich in human interest.

SUMMARY

The Flesch formulas for assessing the readability and human interest of writing were applied to twenty-five articles from five collections of readings in psychology. The articles were found to differ widely: some were readable but lacked human interest; others lacked both; a few were both readable and rich in human interest. Although there has been some improvement in these areas among current readings assigned to beginning college students, the student would still find the majority of the selections difficult and dull.

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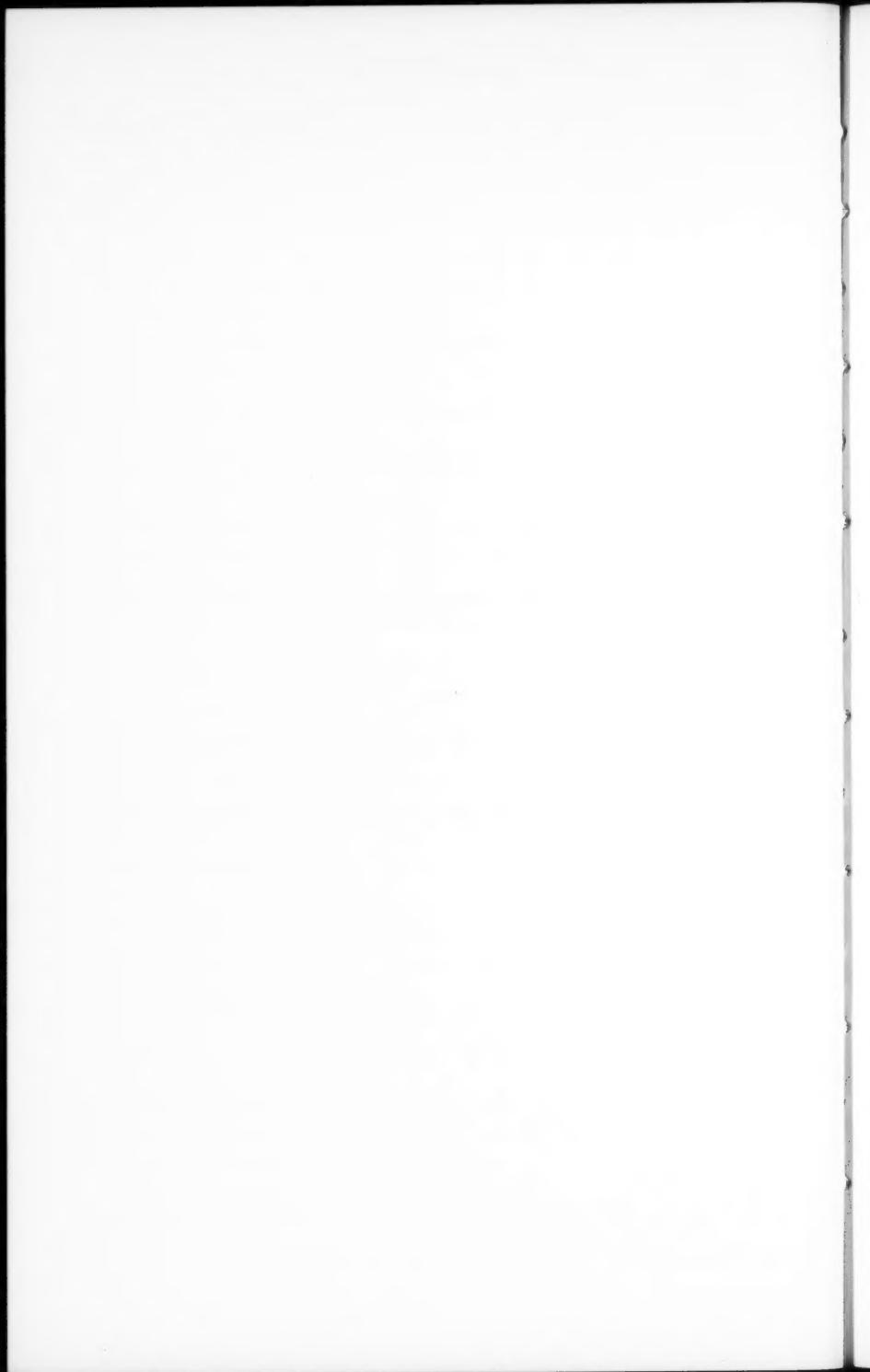
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SITUATIONAL DETERMINANTS OF AFFECTIVE REACTIONS TO PERSONS

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In their study of social interaction behavioral scientists must choose to emphasize either determinants within the participants or determinants inherent in the structure of the situation within which the participants act. Prediction on the basis of measures of personality is appealing, since these variables are trans-situational and relatively enduring. However, prediction according to situation allows us not only to study what already exists, but also to make inferences about the way in which the personal response tendencies were learned. For instance, if we find that a certain situation produces dominant behavior in people, then we may hypothesize that learning which has taken place in situations similar to this one lies behind the personal characteristic of dominance.

What is the nature of the process through which one comes to place value on the characteristics of other people? We suggest that these characteristics acquire value through generalization of the consequences of one's own goal-directed activity. If while interacting with another person one achieves reward, the positive affect associated with the reward generalizes to the perceived characteristics of that person. In this paper we offer a classification of the ways in which two persons, the Subject (S) and the Other (O) can interact in terms of a goal, and examine the implications of this classification and others for the response consequences of the S and the O. We hope to show the situations in which S will achieve his reward and thus come to value positively the characteristics of O.

Exclusive and Non-exclusive Goal Situations

An exclusive goal situation is one in which the attainment of his goal by either S or O excludes the other person from attaining his goal. A non-exclusive goal situation is one in which both S and O may attain their goals.

In a non-exclusive goal situation both S and O may achieve their goals, and if they do the positive affect associated with goal attainment will generalize to the perceived characteristics of both persons. But in an exclusive goal situation the characteristics of only one person will thus acquire positive value: the characteristics of the person who does not attain his goal will acquire positive value for the person who does.

EXCLUSIVE GOAL SITUATIONS



NON-EXCLUSIVE GOAL SITUATIONS

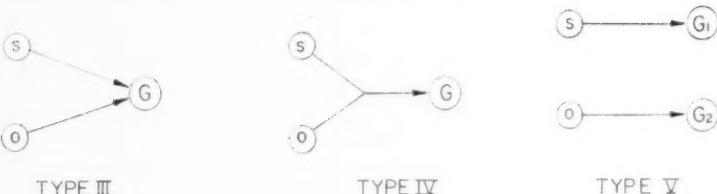


Fig. 1. Exclusive goal situations. *Type I:* S and O striving toward the same goal. *Type II:* S and O striving toward different goals which are mutually exclusive.

Non-exclusive goal situations. *Type III:* Either S or O, or both, may attain the same goal. *Type IV:* Both S and O must attain the same goal for either to do so. *Type V:* S and O may achieve different goals in the same interaction, provided that they are not mutually exclusive.

Cooperative and Competitive Interactions

We will define a cooperative interaction as one in which both S and O may attain the same goal or different goals. A competitive interaction is one in which S and O may not achieve the same goal but may achieve different goals. Cooperative interactions could occur in goal situations of Type III, IV, and V, and competitive interactions in goal situations of Type I, II, and V. S could be rewarded while in association with O in any of the cooperative situations, but in a competitive relationship he could be certain of reward only in goal situation V.

Thus S has more likelihood, other things being equal, of achieving his goal in a cooperative interaction with O than in a competitive one with him; and since we assume that the consequences of S's goal responses will in some degree attach to O, we can predict that S is more likely to evaluate O positively following a cooperative interaction with him than following a competitive one. Furthermore, on the basis of his history of response consequences S is likely to seek out and to try to establish cooperative relationships rather than competitive ones, since participation in cooperative ones maximizes his chances for reward.

This is assuming that in a sample of competitive relationships he would win sometimes and lose sometimes, but that he would almost always be rewarded in a cooperative relationship. We should note in passing, however, that an exceptionally competent individual may be able to win reliably in exclusive goal situations, and that the rewards there may be more desirable than the ones available in non-exclusive goal situations. Thus we might expect the very competent person to prefer goal situations I and II.

Similar and Opposite Rewards

We must distinguish the case in which *S* and *O* are rewarded by the *same* sort of events or goals (goal situations I, III, and IV) from the one in which they are rewarded by *different* sorts of events or goals (situations II and V). Provided that in the second case the rewards of *O* are in some sense opposite to those of *S*, this is another way of stating the similarity-complementarity contrast.

According to the similarity hypothesis it is rewarding to *S* to associate with an *O* who is perceived as having characteristics which are like those of *S*. The complementarity hypothesis states that it is rewarding to *S* to associate with an *O* who is perceived as having characteristics which are opposite to those of *S*.

In a cooperative interaction similarity could be present when *S* was rewarded in Type III, IV and V goal situations, and complementarity could be present in Type IV. Reward value would generalize to similar and to complementary characteristics accordingly. The Type V situation is the only one in which *S* could *consistently* be rewarded in the presence of complementary characteristics.

In a competitive interaction complementarity could be rewarded for both *S* and *O* only in a Type V goal situation.¹ Complementarity would be rewarded in situations I and II only for the person who achieved the goal.

We can say, then, that similarity of attributes will be rewarded consistently only in non-exclusive goal situations of Type III or IV, and that complementarity of attributes will be rewarded consistently only in Type V. Both similarity and complementarity will be rewarded occasionally in situations of Type I and II, particularly for the more powerful individual. We will further discuss the role of power and competence below.

Real and Imagined Interactions

S responds differently to real and imagined interactions. An imagined interaction resembles the cooperative relationship more than the compet-

1. Provided that both *S* and *O* are free to leave the present interaction and enter others which offer better outcomes, our analysis carries the implication that if a competitive interaction is to endure, the rewards *S* receives must be different from those *O* receives. For a discussion of the importance of alternatives to the present interaction and of involuntary relationships, see Thibaut and Kelley, 1959.

itive one, since in the imagined interaction O is not in any immediate or threatening sense in competition with S for a goal. Through symbolic manipulation S can control in fantasy the outcomes of the interaction. We might even go so far as to say that since S has this autistic control the fantasized O "cooperates" with S in helping him to attain his imagined goal.

We would expect the similarity hypothesis to hold more often in the case of imagined interactions than in the case of real ones. This would be more true of idle daydreams and less true of actual anticipations of interaction. The results of Jones and Daugherty (1959) indicate, however, that we must be more specific, particularly regarding the nature of the personal characteristics under investigation. When their Ss anticipated a competitive interaction with a political stimulus person, the correlation between flattery of the stimulus person and a measure of political *values* (with political *need* held constant) was positive (.576), but the correlation between flattery and a measure of political *need* (with political *value* held constant) was negative (-.667). These findings suggest the need for specificity and caution in our generalizations, and above all for variegated data.

To some extent a person's imagined interactions serve as a model for his real interactions or as a rehearsal for them. Obviously such rehearsal will be effective as preparation for action only to the extent that the contingencies in the imagined interactions are correlated with those the person faces in his real ones.

Personal Power and Preference for Type of Interaction

We alluded above to a distinction among persons in terms of their personal power, this term referring to the ability to control their own outcomes and other's outcomes in the interaction. For our purposes here we will assume that S and O already differ in personal power, and that this characteristic is a function of their histories of reinforcement for acting powerfully as well as a function of such factors as competence, strength, control of scarce resources, etc. We would expect that the person of high personal power would prefer goal situations I and II (the exclusive ones) or situation V, for in these three he can attain his goal without having to share it with the other person. If these are in fact the sorts of real interactions the powerful person comes to prefer, and if in trying to maximize his rewards he pictures himself in these sorts of relationships in his fantasy, then his imaginary interactions will be largely effective as preparations for action. On the other hand, if the person of low power, similarly trying to maximize rewards, pictures himself in imaginary interactions as the more powerful person in goal situations I or II, then his fantasized interactions will not be effective as preparations for action. Usually the less powerful person will attain his best outcomes in the non-exclusive goal situations III, IV, and V.

Personal Power, Similarity, and Assumed Similarity

The attribution of traits to *O*, and the estimation of the amount of any characteristic *O* has, work to a large extent in the service of *S*'s efforts to maximize his own rewards. This is particularly true for imaginary interactions because there is less opportunity in fantasy for negative feedback. In real interactions a manifestly incorrect evaluation of *O* will result in a clash of activities and consequently in a more veridical restructuring.

We have suggested above that the more powerful person will come to prefer exclusive goal situations, and the less powerful person non-exclusive ones, although both types of persons may attain their rewards in a Type V situation. We now further suggest that both persons of high and low power will tend to attribute to the other person those sorts of personal characteristics which if truly present in the other person are most advantageous to *S* in the sorts of goal situations he prefers.

Following a thought developed earlier, in which it was suggested that similarity of personal characteristics could be rewarding in goal situations of Type III, IV, and V, and complementarity in those of Type I, II, and V, and comparing this with our notion that the more powerful person prefers I, II, and V and the less powerful person III, IV, and V, it follows that the more powerful person will most often be rewarded in the presence of complementary personal characteristics, and the less powerful person in the presence of similar characteristics, and that this reward value will generalize accordingly to *O* and his characteristics. In other words, the more powerful *S* will come to like *Os* with *complementary* personal characteristics, whereas the less powerful *S* will come to prefer *Os similar* to himself. Furthermore, we would expect the more powerful *S*, upon meeting a stranger, to assume more complementarity than actually exists, and the less powerful *S* to assume a greater than warranted degree of similarity.

*Initial Evaluations of *O**

Thus far we have tried to show how the personal characteristics of other people acquire value through generalization of the affect associated with the consequences of *S*'s attempts to attain his goal in the interaction. To some extent the value associated with these characteristics attaches to a new *O* in whom *S* perceives them. If in the past *S* frequently has failed to attain his goal when *O* behaved dominantly, the negative affect associated with non-attainment of goal will attach to a new *O* whom *S* perceives to have dominant tendencies.

S also values his own characteristics on the basis of their association with the consequences of his goal behavior. This fact has consequences for his evaluation of *O*; for if a variation in outcomes has resulted in the past from a certain sort of behavior on the part of *S*, the affect associated with that variation will attach to similar behavior perceived in *O*. If *S* has been rewarded for dominant behavior, he will respond positively to dominant tendencies perceived in *O*.

This is true of cooperative and imagined interactions. It is also true of competitive and real interactions, but here O's immediate and present impact on S's outcomes has far more weight. The results of Jones and Daugherty (1959) generally substantiated their hypothesis that a person high in political need would like a political stimulus person more in the abstract than in the flesh. When only one person can attain the goal, similar characteristics in O, particularly ones instrumental to the goal, constitute a threat to S's outcomes, and the negative affect associated with non-attainment of goal will attach to O.

Conclusions

We have presented a classification of dyadic interactive situations in terms of goals and within this framework have discussed several other situational determinants of affective reactions to persons. On the basis of considerations herein developed we have advanced the following hypotheses:

1. In non-exclusive goal situations the personal characteristics of both S and O may acquire positive value for the other person; but in exclusive goal situations only the characteristics of the person who does not attain the goal will acquire positive value for the other person.
2. If personal power is held constant, S will evaluate O more positively following a cooperative interaction than following a competitive one.
3. If a voluntary competitive interaction is to endure, the rewards S receives must be different from those O receives.
4. The similarity hypothesis will hold more often in the case of imagined interactions than of real ones, particularly for similarity of needs and also for personal characteristics which are related to attainment of the goal in the interaction.
5. The imaginary interactions of powerful persons will be more effective as preparations for action than those of less powerful persons.
6. Persons of high personal power will prefer complementarity of personal characteristics, and persons of low personal power will prefer similarity of personal characteristics.
7. Powerful persons will assume more complementarity than exists, and persons of low power will assume more similarity than exists.
8. The generalization of affect associated with own and others' previous goal responses will have more influence on S's present judgment of O in cooperative and imaginary interactions than in competitive and real ones.

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AN EMMERT'S LAW OF APPARENT SIZES¹

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The controversy which began in 1940 between Boring and Edwards and Young over after-image size perception has centered around a lack of differentiation between physical size and apparent size (Boring, 1940; Edwards, 1953; Edwards & Boring, 1951; Young, 1950; Young, 1951). Emmert's law as it is commonly stated in psychological dictionaries relates the size of after-images to the distance to the plane of projection without saying which size (physical or apparent) or which distance (again—physical or apparent) is meant. It became evident that Emmert himself had failed to make this distinction and that there should in fact be two "laws": an Emmert's law of physical sizes and an Emmert's law of apparent sizes (Edwards & Boring, 1951; Young, 1950). Young (1948) has defended a law of physical sizes and at least in the average case has supported it with experimental evidence. Boring and Edwards (1951) hypothesized a law of apparent sizes. Edwards (1953) has demonstrated that a law of apparent sizes fails with the elimination of depth cues, as it should if the hypothesis were correct. More recently Hastorf and Kennedy (1957) and T. G. Crookes (1959) have made experimental contributions; however, they too have failed to make the proper distinction between physical and apparent sizes and the processes by which each must be measured if the measure is to be valid. If one is measuring *physical* sizes, some sort of measurement of *occlusion* is adequate, for all that is being tested is Euclid's law. If one is measuring *apparent* sizes, then a *comparative technique* should be used with all the factors attendant on depth perception inherent in the measure.

In 1948 Young established an Emmert's law of physical sizes for the average case, but thus far no one using a comparison technique has demonstrated that apparent sizes follow Emmert's law.² Furthermore, no one has demonstrated specifically that apparent size is dependent upon apparent distance rather than physical distance. In most instances where depth cues are abundant and dependable, apparent distance approximates closely physical distance; however in an experimental situation it should be possible to manipulate the depth cues so that apparent distance no longer approximates physical distance. It is

1. The following is a thesis submitted to the faculty of the University of Delaware in partial fulfillment of the requirements for the degree of Bachelor of Arts with Distinction in Psychology.

2. Crookes (1959) used a comparison technique; however, his instructions required his Os to estimate the visual angle subtended by the standard stimulus and to select a comparison stimulus which subtended the same visual angle. This E feels that in order to measure apparent size he should have instructed his Os to select a comparison stimulus that looked to be the same physical size as the standard stimulus.

expected in this case that the apparent size of an after-image would be dependent upon apparent distance. If this dependency could be demonstrated, it is felt that it should be incorporated into an Emmert's law of apparent sizes. It is therefore the purpose of this experiment to test the hypothesis that: Within the limits of size constancy, the apparent size of a negative after-image is directly proportional to the apparent distance of the plane of projection.

The experiment was designed in three stages. The first stage was simply a size constancy experiment in which the size of real objects was judged by a comparison technique. The second stage was identical to the first except that projected after-images were used in place of the real objects. It was expected that Stage 1 would establish the limits of constancy judgments which could be made in these particular experimental conditions and that the results from Stage 2, in which after-images were used, would parallel those in Stage 1 and would be accurate within the limits established in the first stage. The third stage was designed to present the optical illusion of depth reversal in such a manner that the apparently more distant projection screen of two which *O* could see to his front would actually be the closer. If the apparent size of an after-image projected on these two screens were to get larger as fixation was changed from the one which was in fact closer to the one that was really more distant, then it could be asserted that apparent size was dependent on actual distance to the plane of projection. If, on the other hand, the apparent size of the projected after-image were seen to get larger as fixation was shifted from the apparently closer screen to the apparently more distant screen, then it could be asserted that the apparent size was dependent, not on the actual distance, but on the apparent distance to the plane of projection.

METHOD

Observers. Five men and five women whose ages ranged from 17 to 21 years were used as *Os*, and each was naive with respect to the purpose of the experiment. All *Os* met a visual acuity criterion of 20/40 (uncorrected) with both eyes and the right eye alone. All measures of acuity were made with an Orthorater (Bausch and Lomb).

Apparatus. Stage 1: The *O* was seated with his head in a head rest. He faced a 71 cm standard square screen made of white poster paper which was placed perpendicular to his line of sight with its mid-point directly in front of *O* at eye level. Centered on the screen was a standard gray square (5° visual angle) with a black fixation point at its center. A comparison screen of white poster paper, 71 cm square, was located at eye level with its mid-point 21 degrees to the right of the first screen and 1.5 meters from *O*. Projecting from its front were two wire supports upon which were set comparison squares. The standard screen was placed at one of three distances from *O* (2, 3, or 4 m). For each distance the standard gray square on the screen

subtended a visual angle of 5 degrees. There were 3 sets of 5 comparison squares (1 set for each standard screen distance) made of the same gray as the standard stimuli. The size of the comparison squares ranged from four to six degrees visual angle in one-half degree steps. The gray used approximated the shade of a negative after-image.

Stage 2: The apparatus was the same as for Stage 1 with two exceptions. First, the standard screen directly to *O*'s front had only a black fixation point at its center since it was a projection screen. Secondly, there was a portable stimulus apparatus consisting of a black box in which was an ordinary 100 watt incandescent bulb illuminating from behind a 5 degree translucent square. The brightness of the square equalled 1600 apparent foot-candles. It was presented to *O* at eye level at a distance of 65 cm. Both Stages 1 and 2 were conducted in the same room and shared similar parts of the apparatus. The room was illuminated by two fluorescent lights which provided the illumination for the screens. This brightness was approximately 12.5 apparent foot-candles.

Stage 3: This stage was conducted in a light-tight room with *O* seated with his head in a head rest and an eye patch over the left eye. He looked through two reduction screens, one at 7 cm and another at 64 cm. What *O* saw was designed to create the optical illusion of depth reversal. There were two projection screens visible, each made of white poster paper and having a black fixation point at its center. The screen to the right (Screen R) was 71 cm square and 3 meters from *O*. The screen on the left (Screen L) was 30.2 cm square with a 2 x 25 cm notch cut out of its lower right hand corner and was 2.5 meters from *O*. Screens R and L were so positioned that the notch on Screen L made Screen R appear to be interposed between Screen L and *O*. The illumination of Screen L was 7.5 apparent foot-candles and of Screen R, 30.0 apparent foot-candles. After-images for this stage were generated by the same apparatus used in Stage 2. The apparatus was placed so as to occlude *O*'s view of the projection screens until the stimulus apparatus was removed. The after-images were measured with two sets of calipers, each made to the same scale as its screen. The calipers consisted of two vertical bars held parallel to each other with an adjustable gap between them.

All measurements of brightness were made with a Macbeth Illuminometer.

Procedure. The *O* was first given the tests of visual acuity.

Stage 1. *O* was seated with his head in the head rest and the two screens were pointed out. He was then given the following instructions:

Notice the gray square or the screen directly in front of you. Your task will be to tell me which of the series of gray squares which I am going to place on this comparison screen (pointing it out) is the same size

as the one on the screen directly in front of you. By the same size, I mean that when you judge them to be equal, it would be possible to take a ruler and measure them both and they would be the same size. It is important that you keep looking directly at the screen in front of you while making the judgments. You may make an estimate that falls between two of the comparison squares. Are there any questions?

Starting with the screen directly to *O*'s front at 2 m and then moving to the 3 and 4 m positions, two trials were run at each distance, one ascending and one descending. The ascending trials started with the smallest square in the set of 5 comparison stimuli and the descending trials started with the largest.

Stage 2. *O* was seated with his head in the head rest and the projection and comparison screens pointed out. It was then explained to *O* how he would get an after-image. The instructions given in Stage 1 were then given with appropriate modification. Instead of there being a directly produced gray square on the screen directly to *O*'s front, a projected after-image was now in its place. The same judgment of equality of size as in Stage 1 was required. The *O* was permitted to fixate on the lighted stimulus for 1 minute on the first presentation and for 30 seconds any time thereafter when the after-image became weak. The procedure was then the same as for Stage 1.

Twice in Stage 2 when the projection screen was 4 m distant, *O*s judged the smallest comparison square of the appropriate set to be too large. Squares from the next smaller series were then substituted and *O* was permitted to make the judgment using the smaller squares. All the odd numbered *O*s went through Stage 1 first and all the even numbered *O*s went through Stage 2 first. In all instances Stage 3 was last.

Stage 3. Before going into the experimental room in which the distance reversal illusion was set up, the *O* was shown how the calipers would be used to measure the after-image. One arm was pushed across the screen until it reached the far side of the projected after-image, and then the other arm was pushed in until the after-image was just bracketed. The calipers were then removed and the gap measured. The *O* was then blindfolded and taken into the experimental room, seated as before, head in a head rest with an eye patch placed over his left eye. *O* was instructed:

You will get an after-image as you have before and will then project it on the left hand screen of the two you will see in front of you when the stimulus apparatus is removed. I will measure it as I have shown you. Then you will fixate on the screen to the right of it. I will again measure the image. We will then go through the whole process again except that you will fixate first on the right hand screen and then on the

left hand screen. Notice as you change your fixation from one screen to the other whether or not the image changes in size. Are there any questions?

Fixation time was the same as in Stage 2. After all four measurements were made, *O* was asked to say whether or not the after-image changed in size as it went from Screen R to Screen L and to rate it on a five point scale: (1) very much smaller (2) smaller (3) same (4) larger and (5) very much larger. The *O* was then asked which screen was the nearer and which the more distant.

RESULTS

In Stages 1 and 2 *O*'s judgment of apparent size in each situation was taken as the mean of his ascending and descending judgments. The measurement of the physical size of the after-image for each *O* in Stage 3 was taken as the mean of the two measurements taken with the calipers. The data from Stages 1 and 2 are presented graphically in Fig. 1. It made no difference whether Stage 1 or Stage 2 was presented first.

The variability of apparent size judgments was only slightly greater for after-images than for the directly produced squares and then only at the most distant screen position. The difference in variability of measurements of real sizes between Screens L and R was also negligible.

When asked whether the after-image got larger as it went from Screen R to Screen L, none of the *Os* said it stayed the same or got smaller, 60% said it got larger and 40% said it got very much larger. All *Os* saw the illusion, that is, they said Screen L was behind Screen R. Therefore, it can be concluded that Screen L was apparently more distant than Screen R.

DISCUSSION

It is evident from the data that under the experimental conditions in Stage 1 the *Os*' judgments of apparent size approached very closely perfect size constancy. The data from Stage 2 show that the apparent size of the after-images became larger as the distance to the projection screen increased, very nearly approximating the results from Stage 1. However, it is also evident that under these experimental conditions the apparent size of after-images tended to be somewhat smaller than that of the directly produced squares. Whatever the cause of the difference may be, whether it is due to the "realness" of the directly produced square as opposed to the lack of concreteness of the after-image, some accommodative function of the eye not compensated for, or from some other source, at least within these experimental limits it seems to be consistent. The data from Stages 1 and 2 can be taken as evidence that, within the limits of size constancy, the apparent size

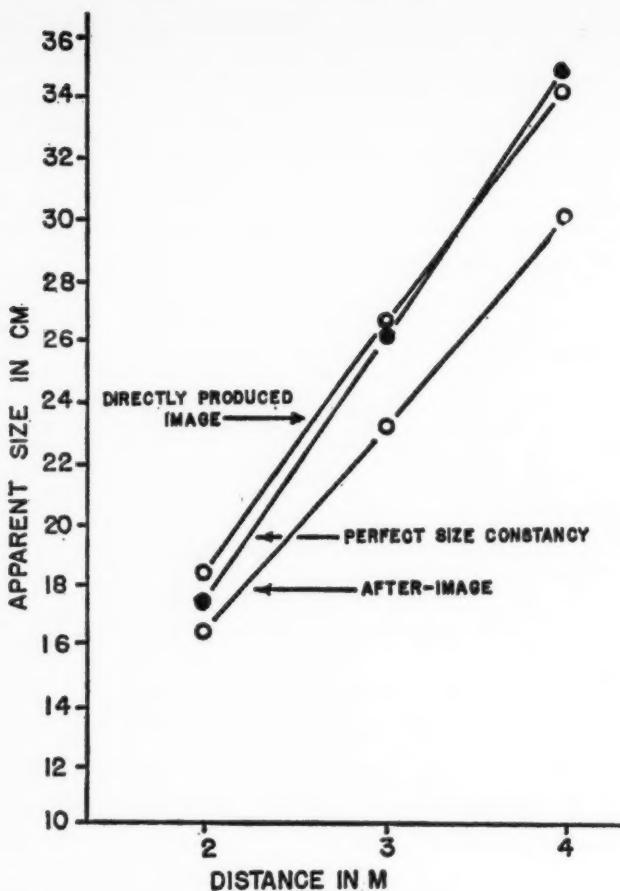


Figure 1. Average apparent size judgments by 10 *Os* of the standard 5 degree stimulus at each of 3 distances. Graphed for comparison are the judgments which would have been necessary for perfect size constancy.

of a negative after-image increases proportionately with increasing distance to the plane of projection.

The data from Stage 3 indicate that for every *O*, even though the measured physical sizes of the after-images did not closely approximate the predicted size, the physical size of the after-image did increase with increasing distance to the plane of projection (Screen L to Screen R) as Emmert's law of physical sizes would predict, while at the same time the apparent size decreased. A contradiction is not involved here, however, because even though the physical distance had increased, the apparent distance, due to the illusion, had decreased.

This evidence indicates that apparent size varies with apparent distance rather than physical distance. It is therefore assumed that the same mechanisms of judgment were working in Stages 1 and 2. The following modification to the previous statement is suggested: Within the limits of size constancy, the apparent size of a negative after-image increases proportionately with *apparent* distance to the plane of projection.

SUMMARY & CONCLUSION

Although an Emmert's law of physical sizes had been established for the average case by Young, a law of apparent sizes had been only suggested by Boring and Edwards. This experiment was designed therefore to test the hypothesis that: Within the limits of size constancy, the apparent size of negative after-images is directly proportional to the apparent distance of the plane of projection.

In the first two stages, similar size constancy experiments were run, one using directly produced squares, the other after-images. In the third stage an optical illusion was devised so that physical and apparent distances to projection screens were unequal. The *Os* were asked to note after-image size changes as fixation was shifted. The results indicate that the apparent size of after-images increases with apparent distance although they tend to be perceived slightly smaller than directly produced squares of the same calculated size. The third stage demonstrated that apparent size of after-images is dependent upon their apparent distance. The data therefore support the hypothesis.

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THE FUNCTIONAL ROLE OF DISCRIMINATIVE STIMULI IN FREE OPERANT PERFORMANCE OF DEVELOPMENTALLY RETARDED CHILDREN¹

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In simple two-choice discrimination tasks, performance may involve either or both of two tendencies: responding in the presence of the discriminative stimulus for reinforcement (approaching the "positive" cue, S^D) and not-responding in the presence of the discriminative stimulus for non-reinforcement (avoiding the "negative" cue, S^Δ). When both stimuli are available in a situation, it is rarely possible to determine which is prepotent, or if both are equally functional. Identical performances could be a function of "approach" alone, "avoidance" alone (assuming a general situational S^D), or some complex combination of the two.

Previous studies of the role of positive and negative cues in discrimination learning (House, Orlando, & Zeaman, 1957; Thompson, 1954) used an ambiguous-cue technique, in which one or the other of the stimulus objects in a two-choice visual discrimination task changes from trial to trial. If the varied cue is functioning in the discrimination, performance should be poorer than in a comparable situation in which both cues are constant. Similarly, if the varied cue is non-functional, performance under the ambiguous condition should be unimpaired.

One difficulty with this approach is that the aspect of "ambiguity" or "novelty" in the varied cue may function as a discriminative stimulus in its own right. For example, some results (House, *et al*, 1957) suggest that Ss learn to avoid the negative cue over a series of trials in which this stimulus is varied. Since reinforcement is always contingent on responding to the discriminative stimulus (S^D) for reinforcement, it is possible that Ss learned to avoid novelty, even under conditions in which the S^D is constant. Inferring that the S^D must be prepotent when performance under the variable-negative condition does not show decrement might be quite inaccurate, since performance could be a function of avoidance of the discriminative stimulus for non-reinforcement (S^Δ) in spite, or because of, its novelty characteristics.

A free-operant situation avoids this difficulty. Since responses are not defined in terms of manipulation of the stimulus objects (as in the

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Wisconsin General Test Apparatus), stimuli may be simply *removed* for short periods instead of being made variable, without altering the opportunity to respond. In addition, test conditions (stimulus removals) can be inserted within a session at points where reinforcement would not ordinarily be forthcoming. This permits withholding of reinforcement during test periods, reducing the likelihood of new learning (e.g., learning to respond to novelty).

The basic rationale for the free-operant stimulus-removal procedure is identical with that of the previously cited research which used the ambiguous-cue technique. If performance remains relatively stable in the absence of one of the relevant discriminative stimuli, its functional role must be small. Conversely, if removal of the stimulus is accompanied by deterioration of performance, the degree of disturbance is probably a function of the relative role of the removed stimulus.

The purpose of the present study is to evaluate the applicability of a free-operant situation for the assessment of the role of discriminative stimuli in simple discrimination. Only effects of removal on performance (maintenance) are analyzed. Similar operations could be applied during acquisition and extinction of discriminative behavior.

METHOD

Subjects

Three developmentally retarded boys served as Ss. Characteristics of the Ss, including age, IQ, diagnostic category, and length of institutional residence, are given in Table 1. Nine Ss from the original sample of 12 were discarded for a variety of reasons, including illness,

TABLE 1
CHARACTERISTICS OF THE THREE SUBJECTS

Subject	CA*	IQ	Diag. Cat.	Residence*
1	16-11	40	familial	1-3
2	16-2	61	unknown	7-0
3	17-6	34	post - traumatic	6-11

* In terms of years - months

failure to learn the discrimination, or extinction before the series of tests were completed. The long acquisition stage necessary to reach discrimination performance criterion for Ss in the study (4, 10, and 8 sessions, respectively) can be considerably reduced by a technique for the rapid establishment of multiple schedule performance (Bijou & Orlando, 1961).

Apparatus

The experimental situation has been described in detail elsewhere (Orlando, Bijou, Tyler, & Marshall, 1960). Briefly, it consists of a room equipped with two response panels spaced 4 feet apart, separated by a chute for receiving reinforcers (small pieces of candy). Two stimulus jewel lights (one red and the other blue) and a plunger-type Lindsley response device are mounted on each panel. Reinforcements are delivered automatically *via* the chute according to a programmed schedule, while the lights are illuminated alternately (only one color at a time on each panel) by a separate interval schedule. For example, the situation may be as follows: the red light (S^D) on the right panel and the blue light (S^Δ) on the left panel are illuminated. Responses on the right plunger are reinforced according to the schedule while responses on the left plunger are not reinforced. After a short interval (e.g., 2 minutes), the red light and the reinforcement schedule shift to the left panel as the blue light and extinction alternate to the right panel. During this period, only responses on the left plunger are reinforced. These periods alternate regularly during the session.

Procedure

In the initial session, Ss were shown how the plungers worked and were told that candy would occasionally drop into the chute. After the S had earned one reinforcement without help, he was left alone in the room for 32 minutes. Sessions after the first were preceded only by the instruction, "It is your turn to get some candy," and were scheduled every three or four days for the remainder of the study.

For each S, one of the stimulus lights (e.g., red) was arbitrarily designated as S^D and the other (e.g., blue) as S^Δ . The lights alternated regularly every two minutes between the two panels. "Correct" responses were defined as plunger pulls on the panel which exhibited the S^D . During the initial session, correct responses were reinforced on a variable ratio 25 (VR 25) schedule until rate exceeded 20 responses per minute. From that point, and for all succeeding sessions, a schedule of VR 100 was programmed for all correct responses. Technically, this is an interlocked *mult VR100 ext* schedule (Ferster & Skinner, 1957) in which one of the components is available on one plunger and the other is on the second plunger.

Criterion for discriminative performance was 90 per cent correct responses for an entire 32 minute session. After criterion was met, Ss were given a series of test conditions in which a stimulus-removal period was inserted toward the end of each successive session. That is, from the 20th to the 24th minute in each session, one of three test conditions was inserted. In counterbalanced order, each S received two tests each of *positive-cue (S^D) removal* (e.g., red light off and no reinforcement), *negative-cue (S^Δ) removal* (e.g., blue light off and no

reinforcement), and *reinforcement (S^R) removal*. Extinction was programmed during the tests so that new response tendencies would not be reinforced. The reinforcement-alone removal condition acted as a control for the effects of the extinction contingency during the tests of cue function.

RESULTS AND DISCUSSION

Mean per cent correct responses per 4 minute block for two sessions under each test condition are shown in Table 2 for each S. Performance is generally better than 97 per cent for most blocks. Effects of cue-removal can be seen in the sixth block (20th to 24th minutes).

TABLE 2

MEAN PER CENT CORRECT RESPONSES BY FOUR-MINUTE BLOCKS FOR EACH S: TWO TEST SESSIONS UNDER EACH TEST CONDITION ARE AVERAGED IN EACH ROW

S	Removal condition	Four-minute block							
		1 0-4'	2 4-8'	3 8-12'	4 12-16'	5 16-20'	test 20-24'	7 24-28'	8 28-32'
1	S ^D	98.3	99.6	99.4	98.3	99.7	56.5	98.2	98.3
	S ^Δ	97.5	98.9	99.7	99.8	98.1	99.8	99.4	99.1
	S ^R	98.5	99.0	99.6	98.5	98.6	99.8	97.0	99.5
2	S ^D	100.0	98.4	98.5	99.5	98.5	66.3	99.4	98.9
	S ^Δ	100.0	99.7	98.8	99.9	98.3	99.2	98.9	96.1
	S ^R	99.6	99.7	96.8	99.9	99.3	97.8	99.9	99.0
3	S ^D	97.2	97.2	99.3	98.2	99.7	96.5	86.7	83.2
	S ^Δ	97.8	99.0	97.9	98.4	99.1	32.4	96.5	99.0
	S ^R	97.5	98.1	97.9	98.2	98.7	99.0	99.2	99.0

In comparison with the level of performance before and after the test interval, and with the other two test conditions, per cent correct responses shows a sharp decrease during positive-cue (S^D) removal for Ss 1 and 2, and during negative-cue (S^Δ) removal for S 3.²

These effects are shown graphically in Figs. 1 through 3, which present representative cumulative response curves for the criterion session and for each of the three kinds of test session for the 3 Ss, respectively. The curves for left and right plungers are shown separately although they were recorded simultaneously. The event-line beneath each cumulative record indicates the location of the stimulus lights (e.g.,

² During test intervals, "correct" responses are defined as plunger-pulls on the panel which normally would exhibit the S-D and lead to reinforcement.

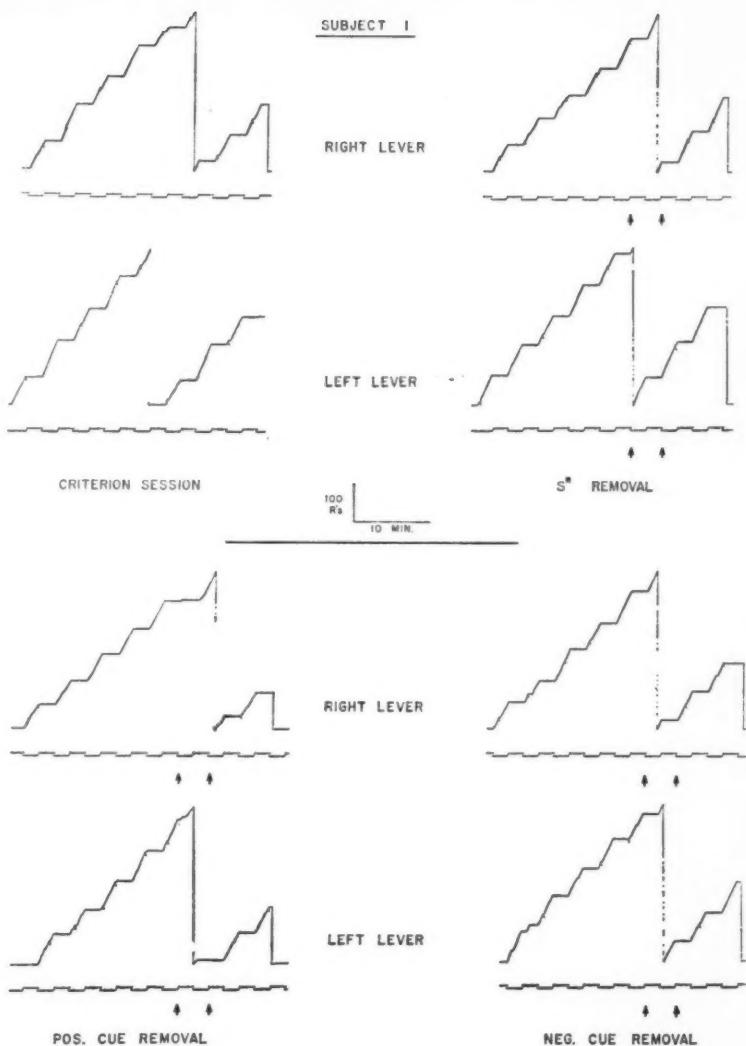


Fig. 1. Representative cumulative response curves for S 1, showing performance during criterion session, S-R removal, positive-cue removal, and negative-cue removal. The start and end of test periods are indicated by small arrows directly beneath the event-line which records cue alternation.

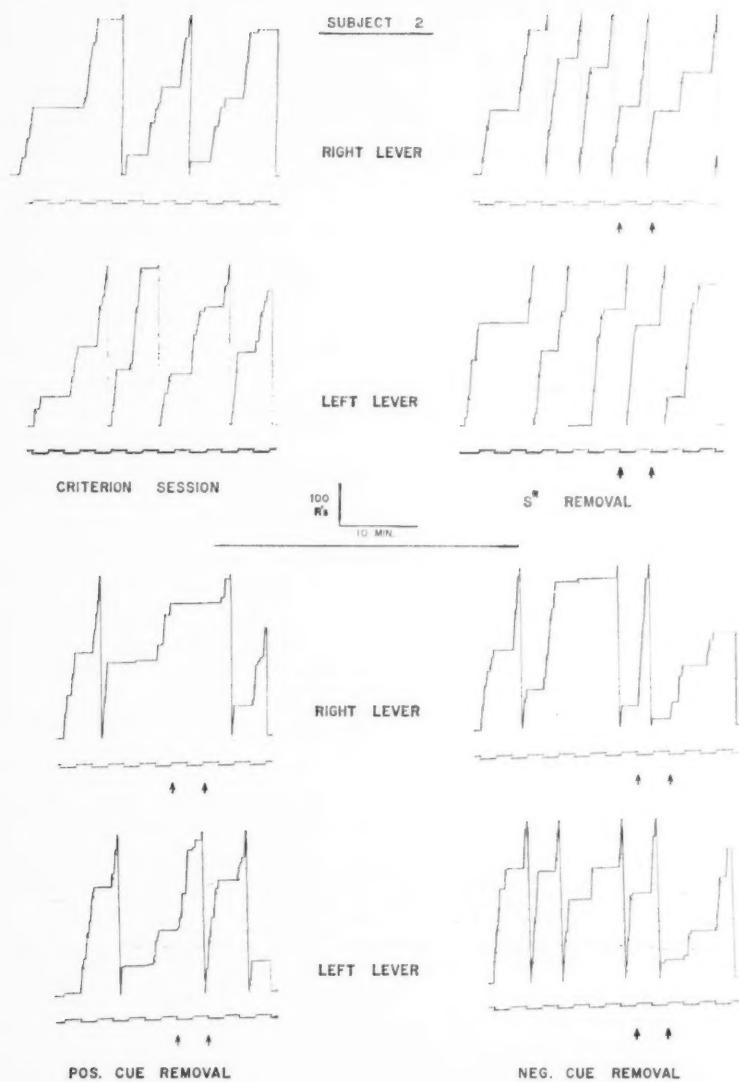


Fig. 2. Representative cumulative response curves for S 2 during criterion and test sessions.

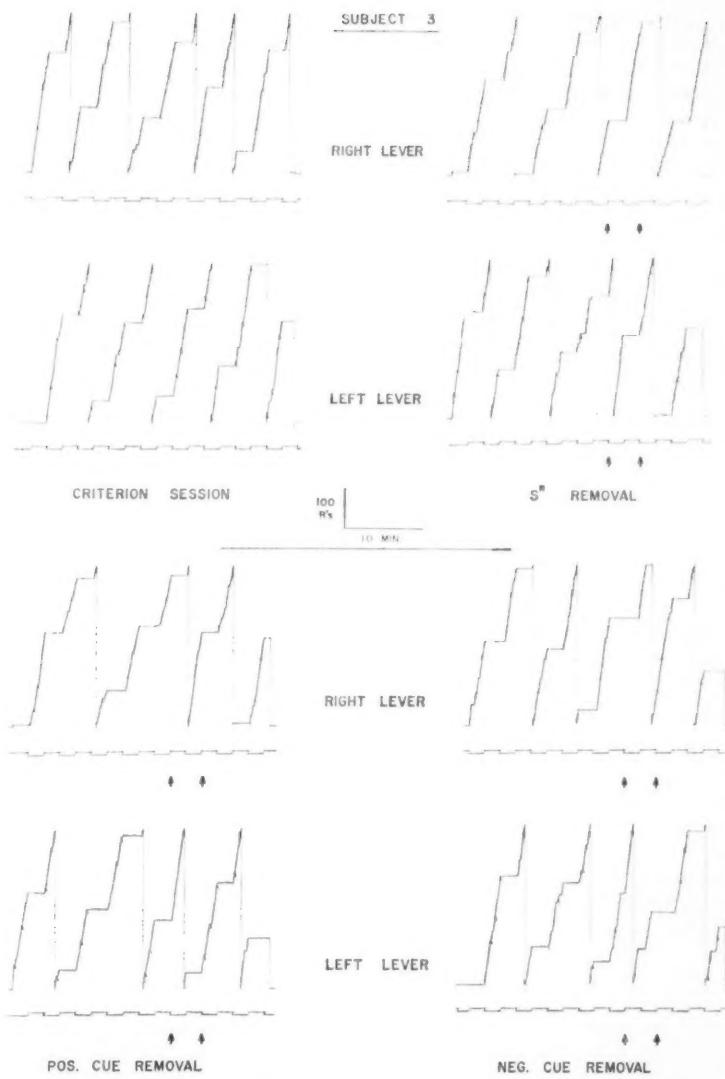


Fig. 3. Representative cumulative response curves for S 3 during criterion and test sessions.

S^D is on the left panel when the event-line is in a depressed position under the left-plunger record and on the right panel when the event-line is depressed under the right-plunger record). Beginning and end of test periods are indicated by small arrows directly under the event-line.

The cumulative curves show uniformly stable discriminative performance during the criterion session and the test sessions in which reinforcement alone was removed. S^D periods are accompanied by responding at high rates while S^A periods are characterized by the absence of responding, indicating that plunger alternation occurred promptly on occasion of stimulus change. This pattern is also apparent in Ss 1 and 2 during negative-cue removal and in S 3 during positive-cue removal. These removal conditions, respectively, did not produce any decrements in performance. However, decrements did occur during S^D removal for Ss 1 and 2 and during S^A removal for S 3. In all 3 cases, poorer performance during these test intervals seems to be a function of failure to *change* plungers promptly when the remaining stimulus alternates in the middle of the test period. Performance recovered to the previous high level when the functional cue was re-presented for the final 8 minutes of the sessions.

From these data, it seems that not all available discriminative stimuli are functional in discrimination performance with these Ss. For this situation at least, one kind of cue is not only sufficient, but is *exclusively* functional, and not all Ss become conditioned to the same kind of discriminative stimulus. Lack of the prepotent cue during the test periods did not produce enough change in the situation to eliminate responding, but only interfered with appropriate behavior change. Continuing responding in the absence of the functional cue possibly may be attributed to the VR reinforcement schedule, which generates high resistance to extinction in these Ss (Orlando & Bijou, 1960), and involved enough range and variability to make discrimination of nonreinforcement during the test intervals unlikely.

In conclusion, it has been demonstrated that a free-operant situation may be used effectively to evaluate the functional role of discriminative stimuli in simple discrimination performance, without confounding effects by introduction of novelty or ambiguity in the test conditions. With these techniques, it is possible to study the extent to which behavioral deviation (Ferster, 1960) or developmental retardation may be partially attributable to specific factors of anomalous or restricted stimulus function.

SUMMARY

A free-operant stimulus-removal method for evaluation of the relative functional roles of positive and negative cues in simple two-choice discrimination performance is described and illustrated with data from three developmentally retarded boys. The method avoids the confound-

ing effects of novelty in a restricted-operant ambiguous-cue technique and permits repeated measures on the same Ss. Results suggest that for retarded boys at least, only one of the available relevant cues is functional in discriminative performance and that the kind of cue conditioned is not the same for all Ss.

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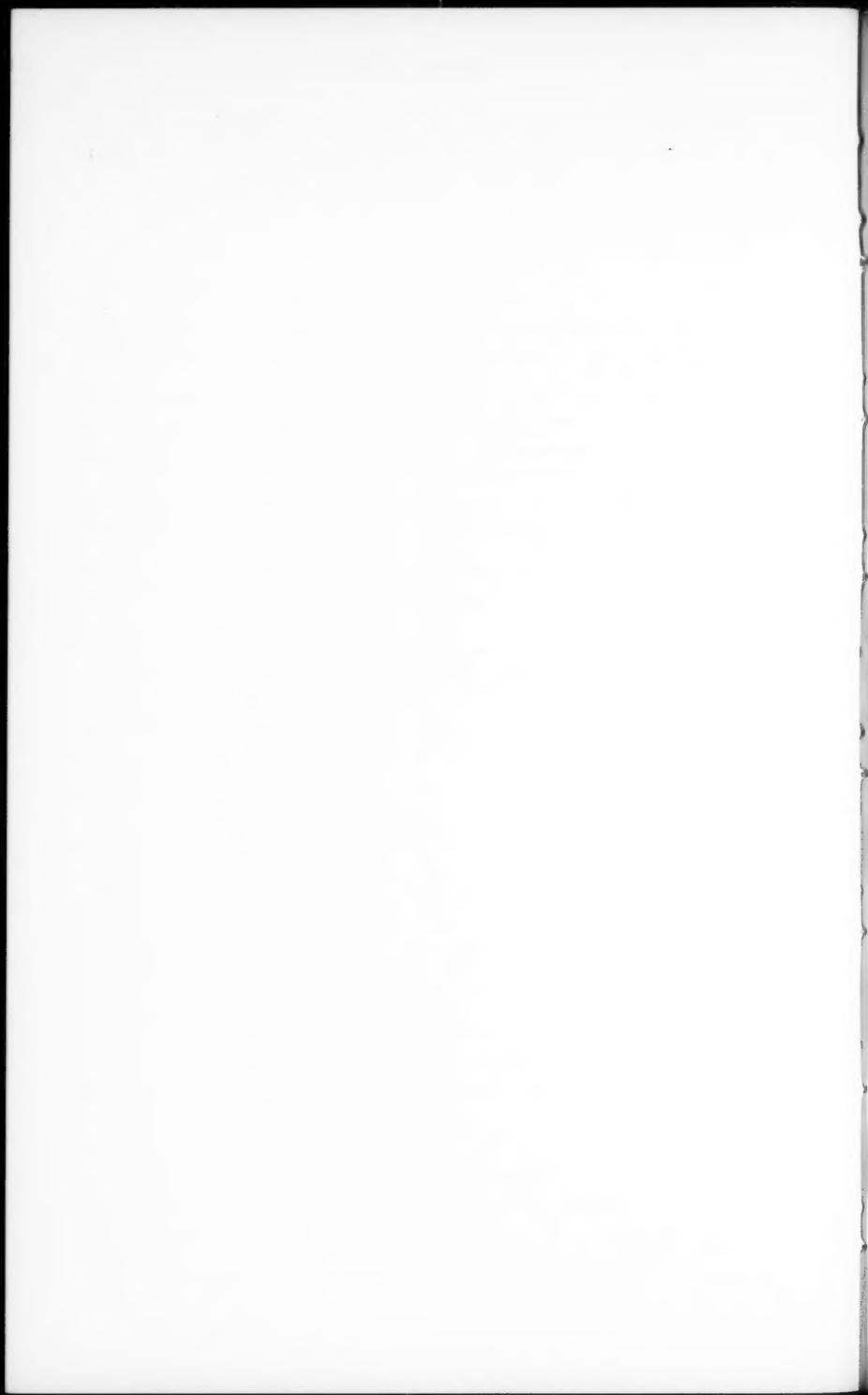
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IS THE "CLICK" A TOKEN REWARD?¹

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Stimuli temporally associated with reinforcement are commonly assumed to acquire two functional properties, viz., a contribution to the stimulus control of the reinforced response, i.e., they serve as discriminative stimuli, and the power to reinforce prior responses, i.e., they serve as secondary reinforcers. Quite recently Hulicka and Capehart (1960) have pointed out that, apart from the fear-as-an-acquired-drive type of experiment (Miller, 1948), the evidence for both of these assumed functional properties is usually derived from extinction data, where the two functional properties are confounded. We cannot tell whether the presence of the secondary reinforcer prolongs extinction because it reinforces the response being extinguished, or whether it is merely one of the stimuli which have come to elicit the response in question.

There have been a few studies designed to isolate these hypothesized functions of secondary reinforcers, in which a click or buzzer, previously associated with primary reinforcement, is presented to see if in the absence of primary reinforcement it will reinforce the acquisition of a new response, like pressing a bar. Using different variations of this technique, Wyckoff, Sidowski and Chambliss (1958), and Hulicka and Capehart (1960) have failed to obtain evidence that the secondary reinforcer actually does reinforce. But on the other hand, Crowder, Gill, Hodge, and Nash (1959) and Zimmerman (1957) do demonstrate the acquisition of new behavior. It is not clear to what experimental variable should be attributed the discrepancy here. All parties to the controversy agree however upon one point, viz., that the hitherto neglected associative function of secondary reinforcers plays a very important role in the determination of behavior. The present paper, too, is concerned with demonstrating that the dominant part played by stimuli associated with reinforcement is their associative direction of behavior rather than any power they may have to reinforce it, or to serve as a token reward.

The stimuli associated with reinforcement are often characterized in another way by expectancy and field theorists: such stimuli are said to become valenced or catheted. According to this interpretation, one would expect the stimulus associated with reinforcement to become preferred, and to be sought after, and one would expect the animal to accept it as a token reward, in lieu of the primary reward with which

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it had been associated. This view of secondary reinforcers (or, more properly, of stimuli associated with reinforcement) is tested in the present study by training rats to respond to two bars in a Skinner-box, and then extinguishing them with a click on one bar and no click on the other. If the click does become a token reward, then *S*, no longer obtaining food, should develop a preference for the bar that gives them the click. The results of this study will help us to clarify this language somewhat, and especially to enable us to indicate what it means for an animal to have a "preference."

EXPERIMENT I

Method

Six naive adult albino rats were used.

The *Ss* were run in an operant conditioning apparatus (Foringer 1102 M) equipped with two bars, (20 gm. pressure), two Davis feeders, and two food troughs. House lights and exhaust fan were run continuously, but no noise generator was used. Reinforcement signals other than the click of the feeder mechanism itself were not presented. The *S* could probably hear response-produced relay noise, but the apparatus used to set up reinforcement was very quiet and no *S* gave any indication of hearing any noise associated with it. The central programming device was a "Percentage Timer," a recycling clock used to set up reinforcement during a given percentage of its cycle. For example, with a 15 sec. cycle set at 30%, responses are cyclically reinforced for 4.5 sec. and not reinforced for 10.5 sec. The resulting schedule is similar to a variable ratio schedule, since a fairly constant proportion of responses is reinforced over a period of time. It is also similar to a fixed interval schedule, since the animal can reduce the ratio of responses per reinforcement by making appropriate temporal discriminations. Percentage timers are manufactured by the Industrial Timer Corp.; their PC model is particularly convenient since it may be continuously adjusted between 3% and 100% while running, and its dial calibration, against a Standard Electric Timer, is accurate to the nearest percentage point.

Each bar of the Skinner-box was connected through a pulse former, an interlock device, and a percentage timer to its feeder. The interlock device was a response-operated timer which electrically isolated the bar for .5 sec. whenever a response was made. The interlock improves the response rat reliability by eliminating from the protocol irregular responses like holding, stuttering, and the tendency to pile up food during the time when reinforcement is available.²

² Preliminary investigation had shown that, for *Ss* trained with an interlock, a .5 sec. dead time gave a response rate nearly equal to that obtained without the interlock. Shorter dead times yielded higher rates, because of holding, whereas longer dead times yielded lower rates, because of missing legitimate bar presses.

Responses and reinforcements for each bar were recorded with a pair of Davis cumulative recorders. One of these recorders was operated in the usual manner to record overall response rate; the pen was moved by a response on either bar. The other recorder was arranged so that a response on one bar moved the pen, and a response on the other bar advanced the paper. (The paper advance stepping motor was built on order by Davis Scientific Instruments.) Thus, the slope of the resulting curve gave a continuous record of the differential response rate on the two bars.

The Ss were maintained at 80% to 85% of their initial body weight, and were run once a day in 25 min. sessions. In the first training session the bar press response was shaped up under continuous reinforcement on one bar. In the second session S was switched over to continuous reinforcement on the other bar. In the next four training sessions the pay off probabilities were switched once within each session, and were brought progressively closer together between sessions, until by the seventh training session each S was working with 30% on each bar and was switching back and forth between bars at a steady, high rate.

On the eighth day all Ss were extinguished with the click on one bar and no click on the other. The click was programmed in the same way that food plus click had been during training, viz., with a pay off probability of 0.30. The extinction conditions were counterbalanced across Ss so that half of them had the click on their preferred side and half on the unpreferred side. On the ninth and tenth days, Ss were reconditioned under the same conditions as before, and re-extinguished, this time with the click and no click conditions of the bars opposite to what they had been on day eight.

RESULTS

If S pressed the bar randomly in time, then it would have 30% of its responses reinforced. However, S spent some time pressing the bar during that part of the cycle when it might more profitably have been pressing for food. The ratio of reinforcements to responses actually observed was $.218 \pm .015$. Throughout training the percentage timers were synchronized randomly, so that at any time S was as likely to be reinforced from pressing one bar as for pressing the other. Thus, a good deal of shifting from one bar to the other was to be expected. There was a good deal of variability in this aspect of the behavior, but the median animals switched bars 71 times during the last 25 min. training session, which was, on the average, once every 21 sec., or after 10 responses on one bar. In the present study one of the most important aspects of the behavior is the linearity of the cumulative record indicating the constancy over time with which S distributed its responses between the two bars. For the sake of convenience we may call the pro-

portion of responses on the left bar the "bar bias." The optimum training procedure may be described as one that leads to a relatively constant bar bias for a given animal, preferably near .50. This was the case; the median change in bar bias (without respect to sign) between the first and last 12.5 min. of the last training session was only .08, and the mean bias for each *S* ranged between .29 and .69, with a mean of .45. Thus, the training procedure provided a stable base line of performance against which the effect of click vs. no click in extinction could be evaluated.

During extinction each *S* had no click on one bar and a click 30% of the time on the other. All six *Ss* showed a shift in bar bias in the direction expected on the basis of the token reward interpretation, up for those with the click on the left and down for those with the click on the right. Upon reconditioning and re-extinction, with the click and no click sides reversed, five of the six *Ss* again showed a shift in bar bias in the direction predicted from the token reward interpretation. The joint probability of two sign tests indicated that the trend is highly significant. The median size of the shift in bias was .16 in the direction favoring the token reward interpretation.

DISCUSSION

The token reward interpretation of secondary reinforcers would seem to be supported by these results, but one further observation suggests another interpretation. During extinction it was found that when *S* received a click on one bar it pressed that bar again, rather than switching bars, approximately 90% of the time. (This figure is only approximate; all but three of the extinction sessions had been run before *E* started to watch the animals in the Skinner-box to see what was going wrong!)

Consider the situation confronting the animal when the reinforcement programming clocks are in random phase with each other, as they were in the present situation. Suppose *S* has just been reinforced on one bar. Then if it continues to press the same bar it has a good chance of collecting another reinforcement during the 4.5 sec. part of the cycle when the timer pays off. But if *S* switches bars, then it has only a .30 chance of obtaining reinforcement. In other words, the click (and the delivery of food) is an S^D for pressing the same bar and an S^Δ for switching bars. A similar analysis applies for the situation following non-reinforcement. With the present procedure *S* is in a situation where it is differentially reinforced for perseverating after a click and switching after no click, and it is unnecessary to make any assumptions about token rewards.

EXPERIMENT II

The second experiment was run to provide further evidence for

this interpretation. The changes from the first experiment consist in arranging the percentage timers so that they were always half a cycle, or 7.5 sec., out of phase, and installing the interlocks so that *S* could never obtain more than one reinforcement from one bar during a cycle. Under these conditions, if *S* perseverates on one bar when it has received a reinforcement, then it must wait nearly 15 sec. for another one, whereas if it switches bars, then reinforcement may be obtained within 7.5 sec. Thus, in experiment II the click (or delivery of food) is a S^D for switching bars and an S^A for pressing the same bar again.

The *Ss* from the first experiment were run again for six days under the new conditions to provide an opportunity for the new reinforcement contingencies to gain control over the behavior, and they were then extinguished, again with the click present 30% of the time on one bar and never present on the other bar. The side on which the click was presented was counterbalanced across *Ss* according to their retraining bar bias.

RESULTS

During extinction all *Ss* showed a shift in bar bias *away from* the side that had presented the click. Under the new reinforcement contingencies the mean percentage of bar switching following reinforcement rose during the six days of training from .10 to .58. It is also of interest to note that the total amount of switching increased during training, and also that the bar bias moved closer to .50 (range: .42 to .59). The median size of change in bar bias (taken without regard to sign) was .14 as against .16 in the first experiment.

DISCUSSION

The view that the Skinner-box click is a token reward implies that *S* would work for it in lieu of food, and would choose to press a bar giving a click in preference to a bar not giving a click. Such an interpretation presents many conceptual difficulties. For example, does "choosing" mean any more than that one response is more probable than another, or that one stimulus controls the response rather than another? If more is meant, then what? The present results suggest that the preferred bar is merely the one the pressing of which has become conditioned to the prevailing stimulus conditions. What is meant by the animal pressing "for" the click? It must mean that the click reinforces the response. There was no evidence in the present study, however, nor in previous ones (Wyckoff, et al, 1958; Hulicka and Capehart, 1960), that the click reinforces the response that produces it *provided* that the role of the click as a discriminative stimulus is experimentally isolated. The present results indicate that the Skinner-box click can be used as a discriminative stimulus for making either the same response that produced it or for making an alternative re-

sponse. In conclusion, in spite of all that has been said about the click as a secondary reinforcer, it now seems plausible to suppose that the click does not reinforce at all, but is just another stimulus in the situation.

SUMMARY

Rats were trained under two sets of conditions to press two bars for food. Under one condition Ss were differentially reinforced for perseverating on the same bar after a reinforcement, and under the other condition they were differentially reinforced for switching bars after a reinforcement. Then all Ss were extinguished with a click on one bar and no click on the other. It was found that bar "preference" depended upon the bar's cue function rather than whether or not it presented the click.

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THE CONDITIONING OF FEAR TO INTERNAL STIMULI¹

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Evidence from experiments by Hull (1933) and by Leeper (1935) indicates that cues due to deprivation can become associated with distinctive instrumental responses. In these studies, rats learned to choose one path of a maze when hungry and an alternate path when thirsty. Since no differential external cues were provided, it must be assumed that the internal cues of the two deprivation states were sufficiently distinctive so that different instrumental responses could become conditioned to each. Furthermore, it has been shown (Brown and Jacobs, 1949; Brown, Kalish and Farber, 1951; Miller, 1948) that an emotional response, presumably fear, can become associated with external stimuli and can exhibit the reinforcing and energizing properties of drive. Recently, some evidence has been adduced (Otis, 1956) to indicate that fear can be conditioned not only to external cues but also internal cues due to hunger or partial satiation and that this fear can affect consummatory and emotional behavior.

In the present experiment, an attempt was made to confirm this finding that fear can be conditioned to internal stimuli, and to see whether the presumed drive increment due to fear would facilitate learning in a straight alley and intensify the magnitude of an unconditioned startle response to an auditory stimulus.

METHOD

Apparatus

Apparatus included fear-conditioning boxes, a stabilimeter, and a black straight alley with a wire mesh cover. Each box was 1 ft. square, and had a hinged cover, a clear glass front, and a grid floor capable of delivering 1 ma. Protruding into the chamber from the cover was a 7.5 watt blinking bulb (on for .5 sec., off for .5 sec.). The alley, which was 42 in. long, 5½ in. wide, and 4 in. high, contained a starting box and goal box (each 12 in. long), both of which were separated from the runway by guillotine doors. The goal box contained an aluminum feeding tray.

Closing the lid of the starting box started a Hunter Klockounter. When *S* passed the starting box door, a photoelectric circuit stopped the

¹Condensed from an M.A. thesis done at the State University of Iowa. I am greatly indebted to Leon S. Otis and Judson S. Brown for their guidance throughout the course of this study, and to Dee W. Norton and Harold P. Bechtoldt for statistical advice.

first timer and started a second one. The second timer stopped when *S* passed the goal box door.

The stabilimeter, consisting of a spring-loaded sensing device, recorded the amplitude of startle response. At the top of this device was a plywood confinement box, 7.75 in. long, 3 in. wide, and 6.5 in. high. Any loud noise would elicit a startle movement from a rat, causing the box to move downward momentarily. The box was connected to an inkwriting lever, the deflections of which were recorded on a moving paper tape.

The startle stimulus was a paper-punching toy pistol, which *E* fired above the confinement box. Any pen deflection immediately following the shot was recorded as a startle response. The varying weights were taken into account by adjusting the device to obtain the same baseline for each rat. Also with this adjustment approximately the same degree of movement was required to produce the same amount of needle deflection for all *Ss*.

Subjects

Ss were 36 male and 25 female naive hooded rats, 41 days old at the beginning of conditioning. They were randomly divided into 3 groups of 18, 21 and 22, each containing the same proportion of males and females.

Procedure

Conditioning consisted of shock in a grid box following either food deprivation or ad libitum feeding ("satiation"). All groups were maintained on a 24-48 hr. deprivation-satiation regimen. They were deprived of food for 24 hrs., then permitted ad libitum feeding for 24 hrs., then deprived 48 hrs. followed by ad libitum 48 hrs., then back to a deprivation of 24 hrs., etc. The two different time periods were used to reduce the possibility of temporal conditioning (e.g., a CR occurring every 24 hrs.). Group D (*N*=22) always received its conditioning following the 24- and 48-hr. deprivation periods. The members of Group S (*N*=21) were shocked following the 24- and 48-hr. ad libitum feedings; and Group C, control (*N*=18) received only adaptation trials (i.e., shock was not administered) in the grid box. When run in the grid box under conditions other than the one scheduled to be associated with shock, *Ss* received adaptation trials.²

At the beginning of the second minute of a conditioning trial, the blinking lights were turned on. At the end of that minute, the lights were turned off, with or without shock, depending on the group being run. A shock of .5 sec. duration was given just before the termination of the light. The light was merely intended to enhance the distinctiveness of the conditioning situation, constituting part of the total stimulus

² The conditioning procedure was conducted by Leon S. Otis.

complex composed of internal stimuli, cues of the box, grid, etc. During adaptation trials, it was expected that the fear response to all stimuli except the internal cues would undergo extinction. Since the internal cues on these trials were different from those during training, fear conditioned to the latter cues would not be expected to extinguish.

Ss were returned to their cages following the third minute in the grid box. All groups received 80 trials (1 daily) prior to the post-conditioning tests. Experimental groups received 40 conditioning trials and 40 adaptation trials. The control group received 80 adaptation trials.

Tests were begun on the day following the last conditioning trial. Rats were placed in the alley to run to the goal box for food reward, 2 min. of feeding on a wet mash. Starting and running times were recorded.³ Ss were then returned to the home cages and permitted ad libitum feeding for 1 hr. on wet mash.

The alley tests were conducted after 6 hrs. of food deprivation, for three reasons. First, since this permitted daily testing. Secondly, it was thought that this period was similar enough to 24 and 48 hrs. of deprivation to evoke some of the fear which had presumably been conditioned to these original deprivation stimuli. Lastly, it was felt that this period was short enough that the general drive level of the animals would not obscure any increment in drive due to fear.

Since the internal stimuli of Group D were most similar to the deprivation stimuli during conditioning, it was expected that this group would have the most fear. Assuming that fear would increase general drive, it was predicted that Group D would have the fastest mean starting and running times. Group C should have the slowest times, since it never received fear conditioning. Since Group S had received 40 fear-conditioning trials, though under different internal stimuli, it might still be a more "fearful" group than Group C because of stimulus generalization, and hence would possibly have a higher drive level than Group C.

Test trials in the startle box were conducted following the end of the 1-hr. ad libitum feeding. Each animal was placed individually in the startle apparatus, and was allowed an adaptation period varying randomly from 30 to 60 sec. The gun was fired when the rat was inactive, so that the deflection could be measured from a straight baseline. Since all animals were satiated, it was predicted that Group S would exhibit the greatest mean startle, as their fear conditioning had occurred under satiation. Presumably Group C would exhibit the least amount of startle. Group D was expected to exhibit a greater mean startle than Group C, due to stimulus generalization.

After the startle tests, animals were taken to the home cages and

³This straight alley test was suggested by I. E. Farber.

permitted ad libitum feeding and drinking until the next 6-hr. deprivation period. This procedure was continued for 12 days, thus providing 12 scores for each animal on the 3 response measures.

RESULTS

The 12 scores for each response measure for individual rats were grouped into blocks of 3 and means of the blocks were computed. These were combined to obtain a group mean for each block, and plots for running time and startle response are shown in Figs. 1 and 2. Lindquist (1953) Type I analyses of variance were performed on the data. Randomly selected parts of the data were checked regarding the assumptions of normality of the criterion scores and normality and homogeneity of the interaction effects. In each case the assumptions were satisfied.

Considering the starting time measure, there was a significant interaction effect ($p < .05$) over trials for the 3 groups, and a significant trials effect ($p < .001$). The main effect was not significant. The significant interaction effect is consistent with the hypothesis that Group D had the most fear, Group S the next most, and Group C the least, since this is the obtained rank order of the starting times for the 3 groups on the earlier trials, whereas on the later trials this rank order does not occur. Thus the significant trials-by-treatment interaction, together with the rank order (on the earlier trials) supports the hypothesis. But the interpretation here is not entirely clear, since the interaction is partly due to crossing of the curves in the later trials. The trials effect was obviously significant because of learning.

More striking are the running time results. Here the same rank order was maintained, as can be seen in Fig. 1. Both the interaction effect and the main effect were significant ($p < .05$ for both) as was the trials effect ($p < .001$). The differences were also significant considering Block 1 alone ($p < .05$). Thus both the main effect and the interaction effect confirmed the predictions. The trials effect again would be expected to be significant because of learning.

From Fig. 2 it can be seen that for the startle response measure the predicted reversal in rank order occurred. The reversal is perfect until the last trial, at which point the curves converge. The rank order, from most to least startle, was Group S, Group D, and then C, and this main effect was significant ($p < .05$). As would be expected, the differences on Block 1 alone were also significant ($p < .05$). The interaction effect was not significant, as would be anticipated since there should be little decrease in a startle response to a loud auditory stimulus over only 12 widely distributed trials.

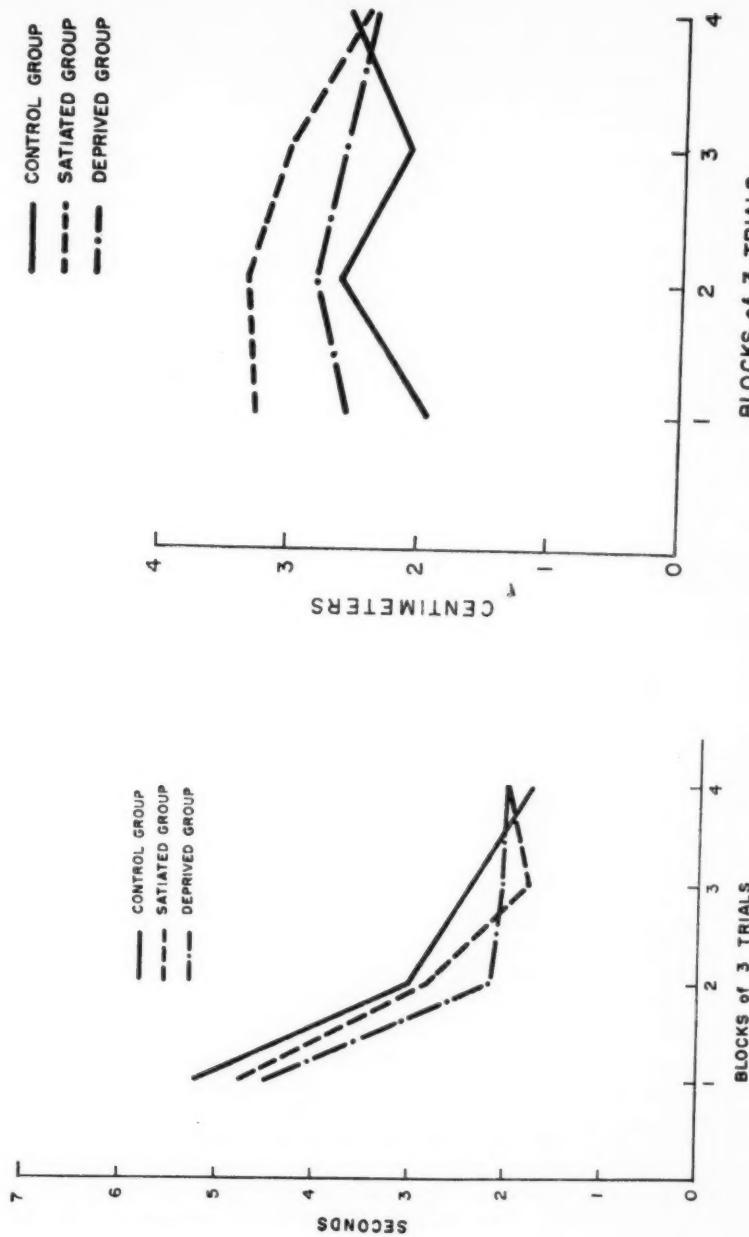


Fig. 1. Mean running times, deprived condition.

Fig. 2. Needle deflection after startle, satiated condition.

DISCUSSION

The hypothesis investigated in the present study was that an acquired response of fear could be conditioned to internal cues associated with noxious stimulation. Any fear thereby acquired would presumably intensify general drive level. Thus, where increased drive would be expected to facilitate performance, the group in which the fear-evoking stimuli were present should exhibit superior performance as compared with groups in which these stimuli were not present.

Analysis of the data indicated that the predictions were confirmed in all 3 response measures, though somewhat unclearly in one. Considering the latency measure, it was predicted that Group D would exhibit the shortest starting times. Group S, though never shocked when deprived, should have a somewhat higher drive level than Group C, since the fear conditioning of Group S might be sufficient to raise the general drive level of the group, i.e., make it more "emotional" than Group C. Thus, when all animals are under 6 hrs. of deprivation, some conditioned fear should accrue to Group S due to stimulus generalization from the cues of satiation to the cues of 6 hrs. deprivation.

A less feasible explanation would be that since testing occurred at the same time of day as did the conditioning, there might be stimulus generalization resulting from temporal conditioning. This possibility would be minimal since for the experimental groups an equal number of conditioning and adaptation trials occurred at that particular time of day. Furthermore, two different time periods were employed (24 and 48 hrs.).

The significant interaction suggests that the treatments did have an effect in the expected direction, for in the early trials the groups were in the predicted rank order. The differences should be the greatest in the earlier trials before the fear response became extinguished sufficiently to obscure the differences. Thus the fact that the curves converge suggests that the fear was extinguishing, and/or that learning was occurring and a ceiling was being reached.

The same reasoning applies to the predictions concerning running times. Again there was a significant interaction effect, as well as a significant main effect. Thus the rank order held up throughout the trials, although extinction did occur. Again, learning and ceiling effects may have contributed to the interaction.

The same logic is applicable for the startle response measure. The rank order should be reversed, however, since Ss were now tested after satiation. Again the main effect was significant. The lack of a significant interaction indicates that the differences between groups were maintained longer than in the alley tests. In comparison with the other two response measures, this becomes plausible when the testing situation is considered. The starting and running measures were not

obtained under exactly the same deprivation period as was in force during conditioning. Therefore the testing actually occurred at a point on a temporal stimulus generalization curve, at some distance from the points of conditioning. In the startle response measure, however, Group S were tested under nearly the same situation as when they were conditioned, i.e., after *ad libitum* feeding. The two situations weren't identical, as conditioning occurred after 24 and 48 hrs. of *ad libitum* feeding, whereas testing took place after only 1 hr. of free feeding. However, since none of the Ss consumed food throughout the full hour, it may be assumed that in both situations they had reached "satiation." Therefore the fear should extinguish more rapidly in the deprivation tests than in the satiation tests.

It is possible to argue, however, that the reverse would be true since there would only be "partial" extinction in the deprivation test situation, and the extinction of the conditioned fear might therefore take longer. The present results seem to empirically favor the former interpretation, though by no means conclusively.

The present startle response findings are in disagreement with those in the Otis (1956) study, in which the most emotional animals had the *least* startle. Otis notes⁴ that his apparatus was "rigged in such a way that the crouching response (of a presumably emotional animal) placed him at a mechanical *disadvantage*, insofar as maximizing his startle to an unexpected stimulus was concerned. This was shown by forcing unemotional animals to crouch in the startle apparatus by confining them within a small plastic box, before sounding the startle stimulus. Although the quarters were large enough so that the animal could turn around and also lift his forepaws off the floor, it had the effect of inducing a crouch. Confined animals had significantly *less* amplitude of startle as compared to non-confined animals (also non-emotional) that were allowed to roam around the 1 sq. ft. area of the startle chamber." Thus in his study crouching resulted in less startle. Crouching resulting from sheer physical confinement was equalized in the present study as all groups were tested in the same box. Therefore the expected decrease in startle due to this factor was eliminated. The results nevertheless remain unclear, since the emotional animals probably still engaged in more crouching behavior than the less emotional ones, and thus according to the Otis findings should have exhibited less startle.

SUMMARY

The purpose of this study was to determine whether fear could be conditioned to internal cues previously associated with noxious stimulation. It was assumed that any fear thus acquired would raise general drive level. Accordingly, in situations where heightened drive was

⁴ Personal communication.

expected to facilitate performance, it was predicted that the group in which the fear evoking internal stimuli were present would exhibit superior performances as compared with other groups.

The animals were divided into three groups, two experimental and one control. A deprivation-satiation regimen was instituted for a period of 80 days during which fear conditioning took place. Group S received a shock after each satiation period; Group D was shocked after each deprivation period; Group C never received shock. It was anticipated that a learned fear would accrue to Group S during conditions of satiation and to Group D during conditions of deprivation.

The hypothesis was generally confirmed in all three response measures. In tests under deprivation in a straight alley, Group D had significantly shorter starting-times (demonstrated by interaction) and running-times (main effect) with Group S next, followed by Group C. When all Ss were tested after satiation, Group S showed a greater startle than the other groups to an explosive sound.

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RELATIVE EFFECTS OF DRIVE LEVEL AND IRRELEVANT RESPONSES ON PERFORMANCE OF A COMPLEX TASK

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The present study was designed to investigate the relationship between effective drive and competing responses as reflected in performance on a complex task.

The Manifest Anxiety Scale (MAS) (Taylor, 1953) was administered to 180 introductory psychology students at the University of Minnesota. Eighty Ss were then selected for the study on the basis of extreme scores. The low (anxious) MAS group consisted of 40 Ss (22 males and 18 females) with scores of six and below. The high (anxious) MAS group was comprised of 40 Ss (15 males and 25 females) with scores of 19 and above.

The total group of 80 Ss met for a second test period four weeks later and was administered a modified form of the Wechsler Adult Intelligence Scale digit symbol test and the Child-Waterhouse Questionnaire (CWQ) (Child and Waterhouse, 1953). The latter purportedly measures the degree to which a S will react to anxiety with responses that interfere with performance. The instructions for the digit symbol test were as follows:

This is one test in a battery currently being used in a study on perception. The task involves reproducing various configurations. You will have only a limited amount of time to work. Both speed and accuracy are important, so work as rapidly and as accurately as you can. Do not skip any spaces. If you make an error, erase quickly and continue. Work the sample below and make certain you understand what you are to do before opening the booklet to begin. . . . Some time after beginning you will be instructed to check the place where you are working. Do this quickly and continue until the stop signal is given.

Ss were then instructed to turn to the first page of the booklet and begin. After three minutes had elapsed, they were told to mark their place and continue. At the end of four minutes the stop signal was

1. In the absence of the junior author, the senior author accepts full responsibility for the preparation of this report. Now at the University of Kansas Medical School.

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given and the booklets were collected. The Child-Waterhouse Questionnaire was then distributed and administered to the total group.

Ss were divided at the median CWQ (interfering response) score into the following four groups: high anxious, high interference; high anxious, low interference; low anxious, high interference; low anxious, low interference. Scores tabulated from the digit symbol test included the number of responses during the first three minutes, the number of responses during the fourth minute, and the total number of errors.

RESULTS

Interscorer reliability of the digit symbol test was assured by having both investigators score each protocol independently and without prior knowledge of either MAS or CWQ performance. Disagreement occurred in the error scoring of three tests and they were excluded.

MAS Analysis

The means and SDs of high and low scoring MAS groups for digit symbol test performance are presented in Table 1.

TABLE 1
MEANS, SDs AND *T* VALUES FOR DIGIT SYMBOL TEST
PERFORMANCE OF HIGH AND LOW SCORING MAS GROUPS
(High MAS *N*=40; Low MAS *N*=40)

Digit Symbol Scores	High MAS		Low MAS		<i>t</i>
	Mean	SD	Mean	SD	
Uncorrected					
First 3 min.	29.02	9.55	27.87	8.49	1.78
Fourth min.	11.45	4.44	10.55	3.71	.97
Total time	40.47	13.43	38.42	11.54	.61
Errors	3.42	5.39	1.55	2.50	1.97*
Corrected					
First 3 min.	26.57	8.91	26.52	7.74	a
Fourth min.	10.47	4.21	10.35	3.57	a
Total time	37.05	12.62	36.90	10.77	a

* Significant at the .05 level.

^a *t* values not computed.

As shown in Table 1, MAS groups did not differ significantly with respect to number of digit symbol test responses executed during the first or second time interval. An analysis of the difference between groups for total error score yielded a *t* of 1.97 (*P* .05), indicating a significantly greater number of errors made by Ss in the high anxious group. A test of the difference between the number of high and low MAS Ss making errors, however, was not significant.

To ascertain whether differences occurred between high and low MAS Ss erring early or late in performance, separate error scores were computed for the first three minutes and the fourth minute and appear in Table 2.

TABLE 2
HIGH AND LOW SCORING MAS Ss ERRING ON THE DIGIT SYMBOL TEST DURING THE FIRST THREE MINUTES AND THE FOURTH MINUTE OF PERFORMANCE

MAS Groups	Ss erring during the first 3 min. N	Ss erring during the 4th minute N	Total Ss erring N
High	11	15	26
Low	18	4	22
Both groups	29	19	48

Chi Square = 7.09, significant at the .01 level or beyond.

As given in Table 2, the difference between groups was significant at the .01 level or beyond. More high anxious Ss erred during the fourth minute, whereas more low anxious Ss erred during the first three minutes.

CWQ Analysis

Data for digit symbol test performance of high and low scoring CWQ groups appear in Table 3.

TABLE 3
MEANS, SDs AND *t* VALUES FOR DIGIT SYMBOL TEST PERFORMANCE OF CWQ GROUPS SCORING ABOVE AND BELOW THE MEDIAN
(TOTAL N=80)

Digit Symbol Scores	CWQ Above		CWQ Below		<i>t</i>
	Mean	SD	Mean	SD	
Uncorrected					
First 3 min.	29.67	9.21	27.22	8.72	1.21
Fourth min.	11.83	4.36	10.17	3.79	1.99*
Total time	41.50	12.95	37.40	11.81	1.46
Errors	3.28	5.38	1.70	2.61	1.65
Corrected					
First 3 min.	27.83	8.40	25.77	8.21	.83
Fourth min.	10.90	4.17	9.92	3.55	1.12
Total time	38.23	12.02	35.70	11.29	.96

*Significant at the .05 level

Table 3 shows a tendency for Ss scoring above the median on the CWQ to obtain higher digit symbol test scores (cf., high anxious Ss *supra*). Only during the fourth minute of performance, however, does the difference reach an acceptable level of confidence. An analysis of the difference in error scores yielded a *t* which fell short of significance. And when a correction for errors (right minus wrong) was made, the slight difference between groups cancelled out.

An analysis of high and low scoring CWQ groups erring early and late in performance, contrary to findings of the MAS, failed to yield significant results.

The total digit symbol test score corrected for errors of high and low scoring MAS and CWQ groups was submitted to an analysis of variance which failed to yield significant results.

SUMMARY AND CONCLUSIONS

The present study sought to determine the relationship between effective drive level and competing response tendencies as reflected in performance on a complex task. Eighty introductory psychology students selected on the basis of extreme Manifest Anxiety Scale (MAS) scores, were administered a digit symbol type test and the Child-Waterhouse Questionnaire (CWQ).

An analysis of high and low scoring MAS groups revealed that the performance of both was debilitated by the digit symbol task, but that low anxious Ss eventually were able to "recover" whereas high anxious Ss were not. A similar analysis of CWQ groups, divided at the median, suggested a lack of sufficient discrimination between groups. An attempt to differentiate performance on the basis of the MAS and CWQ combined, was unsuccessful. An analysis of variance revealed no significant interaction effect. The absence of any significant contribution of the CWQ to yield predictions beyond those which can be made on the basis of the MAS alone, raises question as to the efficacy of its use.

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The Psychological Record, 1961, 11, 181-186.

PERSPECTIVES IN PSYCHOLOGY

XVII. INTERRELATIONS OF FACT AND VALUE IN SCIENCE¹

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That there is a wide gulf separating scientists from literary intellectuals is a point which Snow (1959) has brought forcefully to our attention. The cultural gap is nothing new, although the rapid development of science has tended to widen it considerably. Skinner (1953, p.9) has suggested that we live in a period of cultural transition. He says, "We have not wholly abandoned the traditional philosophy of human nature; at the same time we are far from adopting a scientific point of view without reservation." Kantor (1953, p.315) has pointed out that since there has been "no thoroughgoing assimilation of the sciences with the other cultural components, there is no comprehensive cultural system." Under the prevailing point of view, science is held to be concerned with the results to be obtained from scientific research but not with the value of the results. Under such circumstances scientists "are unable to further the general advancement of civilization" (Kantor, 1953, p.316).

The gap between the two cultures is traceable in part to two time-honored dualisms. The first, the dualism of mind and body, tends to remove some of man's activities from the domain of science. The second, which may have been derived historically from the first, is the dualism of fact and value. Accordingly, facts and values are placed in widely separated categories which are not amenable to investigation by the same methods. Thus, while science can deal successfully with existent things, it cannot provide an understanding of the meaning of these objects and events. Meanings and values are treated as non-physical happenings lying in a metaphysical realm beyond the scope of science (Kantor, 1953).

THE SEPARATION OF FACT FROM VALUE

Many scientists have taken positions which tend to accentuate the separation of fact and value. Some scientists, who claim to have no concern with questions of value, accept the conventional view that values are somehow more important than events and assert that sci-

¹ This article is based upon a paper presented at the meeting of the American Psychological Association in Chicago, Illinois, on September 3, 1960.

ence can deal only with the more mundane questions of existence. Values are thus placed in a transcendent realm to be dealt with only through philosophy and religion (Kantor, 1953).

Other scientists, while deplored the tendency to locate values in a transcendent realm, follow the logical positivists in regarding values as emotive and therefore not subject to determinations of truth or falsity. To say, "You ought not to smoke" for example, is in effect saying, "I don't like to see you smoke" and as a statement of an attitude or feeling the proposition that one ought not to smoke is neither true nor false. Facts and values are once again placed in widely separated categories.

More philosophically inclined scientists may invoke Moore's "naturalistic fallacy" in order to demonstrate the futility of attempting to treat values in a naturalistic or scientific manner. It is asserted that a value is what it is and that it is never reducible to something else. Hence values lie entirely outside the realm of science. One is tempted to ask, however, whether "yellow" which is mentioned by Moore as being phenomenologically unique therefore lies outside science in the sense that the scientist can make no significant statements about it.²

The important point to be made is that in each of the above cases the assumption is that values cannot be handled in any significant way by science. The result is the reinforcement of the unfortunate cultural dualism symptomatic of which is the gulf which Snow has described as separating science from the humanities. This dualism is stressed by such writers as Colin Wilson, Joseph Wood Krutch, Russell Davenport, Friedrich Hayek, and Walter Lippmann.

SUGGESTIONS FOR BRIDGING THE GAP

In attempting to bridge the chasm some scientists have taken the position that values are simply behavioral facts to be studied like any other natural phenomena. Usually those who take this position regard values simply as the goals of motivated behavior, which are correlated with drives, needs, or desires. The study of such goals, their genesis, and modes of operation would constitute a science of values. Unfortunately, however, this position provides no means for determining whether a particular value is good or bad. The so-called objective position (the value of which should not be minimized) leads easily to the objectionable doctrine of complete cultural relativity. This position can properly be criticized for failing to come to grips with a central issue in value theory, namely the evaluation of values. It is not enough to know that under certain conditions people hold particular values. A more important question has to do with whether the values

² Geiger (1949) presents several other criticisms of the "naturalistic fallacy" and shows how the "fallacy" has been invoked to discredit attempts to formulate naturalistic theories of value.

held are good or desirable. The basic question, then, is whether science can help at this point.

When values are regarded as absolute and ultimate, it is true that they cannot be adequately handled by science. Science can no more inform us regarding ultimate values than it can describe the nature of God or ultimate reality. At the same time, science can be concerned with values in a normative as well as a factual sense. A clue to such an approach is found in Dewey's distinction between that which is desired and that which is desirable (Schilpp, 1939). Dewey has contended that the desirable is that which is desired after sustained inquiry. He further implies that scientific inquiry provides the best basis for determining what is desirable. Of course, Dewey raises many questions for which he does not provide adequate answers. He has, however, provided a sound beginning for a scientific study of values which can go beyond simple, factual description. As Dewey uses the word "desirable" one might ask, "Desirable for what?" Does he mean that some human goals are desirable in and of themselves or does he mean that all values are instrumental, that is, that something always has value for someone or for something? Dewey's discussion of the difficulties of distinguishing between ends and means leads us to believe that he thinks of all values as being in some way instrumental and that he avoids all reference to ultimate values.

Dewey does admit that if by ultimate we mean only the last step in a temporal series then he has himself made intelligent inquiry his ultimate value. But Dewey protests that intelligent action is ultimate in use and function and does not stand because of an "absolute 'inherent nature' making it sacrosanct, a transcendent object of worship" (Schilpp, p.594).

The tendency to seek ultimate values undoubtedly stems from cultural sources. Philosophers have frequently defined ultimate values as the only possible means of avoiding an infinite regress. Suppose, for example, that you are advised by a doctor to diet. You ask, "Why should I diet?" receiving the answer, "To stay healthy." But then you ask "Why should I be healthy?"—"To enjoy longer life," etc., each answer giving rise to a further question except when the answer is given in terms of an ultimate value. The argument loses force, however, when looked at psychologically rather than logically. In practice we do accept certain values, such as honesty, health, and wisdom, with little question and it is worth noting that concerning such values there are few disagreements. Disagreements in the field of values are much more likely to arise over the means to be employed to reach a particular goal. Questions of means to achieve a stated or implied end can be put in the "if-then" form and dealt with scientifically.

Dewey is certainly not the only scientist or philosopher to suggest how science can conceivably come to grips with values. Geiger

(1958) has given numerous helpful suggestions for a naturalistic approach to values and he refers with approval to the contributions of Menninger, Goldstein, Kardiner, Fromm, Cantril, Sullivan, Bronowski, and others. To his list we might add Herrick, Skinner, Gerard, Kantor, Köhler, Feuer, McGill, and Lundberg.

Bronowski (1959) has made the point that our values are changing concepts subject to the test of human experience. The habit of truth which the West has acquired leads us to avoid absolute statements beyond the reach of empirical test. Values, like scientific concepts, are tested by their consequences.

Feuer's (1955) examination of values leads him to conclude that ethical concepts such as Nietzsche's "will to power" can be refuted if the psychological assumptions on which they rest are found false. All statements about ultimate values are testable when we ask whether they are expressive of or repressive of underlying unconscious desires. A biological value theory is seen to undergird Feuer's approach as it does the positions of Goldstein, Herrick, Gerard, and Skinner. Theories which see human values rooted in man's biological nature may not be altogether convincing but when viewed as tentative conceptual formulations they can be tested as Bronowski suggests. If there are relatively constant factors in human nature which would tend to make some values more desirable for human societies than others, it then becomes possible that science can prescribe in a limited way and not simply describe. Rapoport (1950) has referred to the possibility of establishing, through science, a culture-studying culture emancipated from the limitations of local conditions. And Skinner (1955) has spoken confidently of the application of science to human affairs. Skinner invokes humanitarian values resting in the last analysis upon cultural survival. Skinner recognizes the impossibility of making an absolute evaluation of the survival value of a cultural practice and enters a plea for modifications in cultural design in order to test consequences.

VALUES INHERENT IN SCIENCE

Scientists reared in the objective tradition often fail to appreciate their deep involvement with values in their scientific work. In the first place, a particular set of values undergirds the entire structure of science. As Bronowski (1959) has indicated, the value system most characteristic of the western world has been brought into being to a large degree by the development of modern science. The values which undergird science are necessary to guarantee the very existence of science. A few of these values may be mentioned briefly:

- 1) Individuals should be allowed freedom of inquiry and freedom of choice in the pursuit of their ends.
- 2) Honesty in method and report is imperative if the scientific community is to survive.

- 3) The scientist must make every effort to prevent contamination of his results by bias or prejudice.
- 4) The scientist must be dedicated to the truth, that is, science must never be perverted by the selfish interest of any individual, state, or nation.
- 5) Knowledge must be freely disseminated. It is not in the spirit of science to withhold findings from any interested group.
- 6) We should be prepared to change our goals and values in accordance with new scientific knowledge.

Values not only enter into science at its foundation, but they also enter directly into scientific work at every step of the way, that is, in the choice of problems to be studied, methods to be used, data to be discarded, conclusions to be drawn, etc. Science also provides the best known method for handling instrumental values, that is, as Lundberg (1947) has said, for telling us how to get where we want to go.

THE TEACHING OF VALUES

One might ask what implications the above discussion has for the teaching of values. First of all it should be pointed out that if there is in fact a close connection between fact and value, then we are doing a poor job in our teaching of science. We tend to teach the sciences as collections of facts enlivened from time to time with bits of theory. We fail almost completely to instill in students a sense of historical continuity or a feeling for the interrelationships of various aspects of culture. How many college students, for example, are ever aware of the fact that the world view of Galileo and Newton derives in no small measure from the ancient Pythagorean system of values?

Saddest of all is our failure to imbue the majority of students with the spirit and values of science. The facts learned in a single college course may soon be forgotten. The methods learned may help to develop a greater appreciation for the work of the scientist. But if the student is aware of the values upon which science rests and of what Bronowski calls "the irresistible need to explore" he will take with him far more than facts and techniques. Yet how seldom is the word value even mentioned in the typical science course.

It is the hope of college and university teachers that we can graduate men and women capable of sound judgment. The habit of thinking scientifically is undoubtedly an ingredient of sound judgment. But sound judgment also involves a sensitivity to values and an ability to discriminate between values. Furthermore, the student with a capacity for sound judgment will always remain painfully aware of the

vast areas of human ignorance and will, therefore, cultivate attitudes of openmindedness, tolerance, and tentativeness.

A clearer recognition by scientists of their involvement with value questions should lead to instruction better designed to develop valuesensitized men and women. It is important, too, that we recognize the need for applying the scientific method in the social sciences. Here the spirit of science is every bit as important as it is in the physical sciences. Finally in the teaching of science we should stress the point that the survival of our way of life requires the assiduous pursuit of science and that the continued progress of science demands a society which accepts the values essential to the survival of science itself.

SUMMARY

It has been asserted that science can deal successfully with values in something more than a simple descriptive way, that is, that science should be able to tell us what is desirable even though it cannot prescribe with finality or beyond the context of a given human situation. Many scientists have misconstrued their task and have attempted to avoid all reference to value. When science is properly taught the interrelationships of fact and value in science will be emphasized. One result of such a modified procedure in the teaching of science should be a greater sensitivity on the part of students to the importance of science for an understanding of values and of the significance of a sound value structure for the continued development of science.

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THE INTERACTION BETWEEN CRITICAL FLICKER FREQUENCY AND ACOUSTIC STIMULATION¹

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Ogilvie (1956a; 1956b) has reported studies in which it was found that binocular critical flicker frequency (CFF) became significantly higher when the measurements were taken when *S* was listening to noise than when no noise was present. Furthermore, a fluttering noise produced greater increases than a steady noise, and when the fluttering noise was in-phase with the flickering light, the increase was greater than when it was out-of-phase with the flickering light.

The results are suggestive of central neural mechanisms involving specific and non-specific sensory paths. Their possible importance in this respect justified the effort to produce similar results with monocular vision.

METHOD

Subjects

Thirteen male *Ss* ranging from age 17 to 30 years were used in Exp. I. Thirteen additional *Ss* from the same population of Engineering College students were used in Exp. II.

Apparatus

The light source was provided by an American Electronic Laboratory electronic stimulator. This device was used to produce a square-wave DC pulse which was wired to activate a GE NE-40 neon bulb and to trigger the noise generator when appropriate. This apparatus produces a train of pulses and permits control of the length of the train and the duration, intensity and frequency of pulses within the train. The light source was viewed through a Macbeth Illuminometer which provided an annulus of constant illumination to match the illuminance of the bulb. A 2mm. artificial pupil could be mounted in the eyepiece of the Illuminometer to control pupil size. The Illuminometer was hooded by a black drape to screen the *Ss*' eyes from external illumination.

Random noise was produced by a 2D21 Thyratron tube, amplified

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2. The authors were assisted in the data collection phase by Jay Caldwell during Exp. I and by Marius C. Smith during Exp. II.

by a 6C4 tube, and gated through a 6J6 tube. The grid of this last tube was biased by the pulse of the electronic stimulator. By reversing the polarity of the pulse from the stimulator the noise could be produced either in-phase or out-of-phase with the pulse. A constant negative bias on the grid opened the gate for the steady noise; a constant positive bias cut out the noise altogether. Toggle switches permitted the experimenter to switch rapidly from one noise condition to another. The intensity of the noise was adjusted to a loud but not uncomfortable level. Measured directly at the earphones the sound was 90 db or 0.0002 dyne/sq. cm.

Procedure

The Ss were in the darkened room for approximately 7 min. while they were shown the equipment and the earphones were fitted to them. The purpose of the experiment was not explained. The Ss spent an additional 3 min. under the hood looking at the illuminated annulus while they were being given practice runs before the experiment proper was begun. This period of adaptation appeared to be adequate, since there was no shift in threshold during the course of the experiment.

Each *S* was given six or more practice trials to determine the appropriate range of flicker frequencies. The flickering light was viewed monocularly with the dominant eye. Subjects were instructed to pay no attention to the noise but to respond "flicker" or "steady" depending upon how the light appeared to them. A warning was given before each presentation throughout the experiment.

The method of constant stimuli was used to determine CFF thresholds. Ten flicker frequencies were presented to each *S* under each of four noise conditions. The frequencies were separated in 2-cps steps from the lowest frequency at which the *S* reported "flicker" consistently to a frequency which was 18 cps higher. The stimulus was presented for 2 sec. on each trial.

In Exp. I the intensity of the flickering light was adjusted in such a manner that the threshold obtained for monocular vision with an artificial pupil was approximately the same as that obtained by Ogilvie (1956a) binocular vision without an artificial pupil at 2.42 log mL. In Exp. II the light intensity was adjusted so that it produced a threshold approximately the same as that obtained by Ogilvie without the artificial pupil. As will be noted in the results, CFF is appreciably lower with an artificial pupil than without.

With 4 noise conditions and 10 flicker frequencies within each condition, blocks of 40 trials each were constructed with the 40 types of trials randomized within each block. Ten such blocks representing a total of 400 trials were presented to each *S*. This required an experimental session of approximately 1 hr. per *S*.

In Exp. II each *S* had 10 blocks constituting 400 trials in each of the

two conditions, with and without the artificial pupil. Seven Ss were tested with the artificial pupil one day and without it the second. The other six were tested without the artificial pupil on the first day and with it on the second.

Thresholds were computed by means of the Urban constant process.

RESULTS

The results of the first experiment may be seen in Table 1. There are no significant differences between the four conditions in this experiment.

TABLE 1
EXPERIMENT 1
CFF'S UNDER FOUR NOISE CONDITIONS
WITH ARTIFICIAL PUPIL

Condition	N	Mean	S.D.
No Noise	13	37.77	2.04
Steady Noise	13	37.64	1.85
In-phase Noise	13	37.84	1.75
Out-of-phase Noise	13	38.50	2.53

There are several differences between the procedures used in this study and those employed by Ogilvie other than the fact that his study involved binocular vision while this one studied monocular vision. Ogilvie had used the method of limits, while the present study employed the method of constant stimuli. His flickering stimulus did not have a luminous surround while an illuminated annulus encircled the flickering stimulus in the present study. Finally, Ogilvie did not employ an artificial pupil.

Of these three differences (other than the binocular-monocular one), it seemed that the most significant one might have been the matter of the presence or absence of the artificial pupil. The second experiment reported here was an attempt to replicate Ogilvie's results with monocular vision but without the artificial pupil employed in the first experiment. In addition, the first experiment with the artificial pupil was replicated at a different light intensity. The results of the second experiment may be seen in Table 2.

The results shown for the artificial pupil in Table 2 amount to a replication of the results in Table 1, since both were obtained with the artificial pupil, although the light intensities were different in the two studies.

TABLE 2
EXPERIMENT II
CFF'S UNDER FOUR NOISE CONDITIONS

A. WITH ARTIFICIAL PUPIL

Condition	N	Mean	S.D.
No Noise	13	32.62	6.20
Steady Noise	13	31.94	4.75
In-phase Noise	13	32.34	4.78
Out-of-phase Noise	13	32.78	5.44

B. WITHOUT ARTIFICIAL PUPIL

Condition	N	Mean	S.D.
No Noise	13	38.12	3.43
Steady Noise	13	38.01	3.48
In-phase Noise	13	38.42	3.28
Out-of-phase Noise	13	38.72	3.73

The only difference which reached the 5% level of significance is that between the steady noise and the in-phase noise. Comparison of this difference with others in the same table and with the same difference in Table 1 suggests that the finding is not repeatable.

The only significant difference in Table 2 is that between the in-phase noise and no noise. This difference is significant at the 2% level and none of the others are significant even though there are larger differences in the table. This apparent anomaly is produced largely by a high correlation between the scores in these two conditions which are not present in the other comparisons.³

Thus nothing approaching Ogilvie's results were obtained with monocular vision and with an artificial pupil. Without the artificial pupil one significant difference was found which was in agreement with Ogilvie's binocular results. Other differences he found were not replicated in monocular data. For example, without the artificial pupil the mean CFF for out-of-phase noise was higher than that for in-phase noise which is reversed in direction from Ogilvie's significant finding.

While Ogilvie declined to offer an explanation for his results, they are quite suggestive with respect to current theorizing about

3. The test used is a "t" test for correlated means. The relevant product moment correlations for the three largest differences in the top tables are .71, .86, and .90. The correlation between scores in the instance of the apparently significant difference in Table II between No Noise and In Phase Noise without the artificial pupil is .9937, thus accounting for the "t" value at the 2% level and the smaller difference of .30.

central nervous functioning. The present results failed to find Ogilvie's relationships between CFF and noise with monocular vision and with an artificial pupil. A partial replication was obtained with monocular vision without an artificial pupil. The present results suggest that the mechanism of sensory interaction demonstrated by Ogilvie might operate through the system controlling the size of the pupil. This suggestion seems to impose a degree of caution in the interpretation of the mechanism underlying Ogilvie's results.

SUMMARY

In an experiment reported by Ogilvie it was found that binocular CFF was significantly increased when measurements were taken while *S* was listening to a random noise "fluttering" in-phase with flickering light. A replication using monocular vision and an artificial pupil under the four conditions of Ogilvie's experiment—no noise, steady noise, in-phase noise and out-of-phase noise—yielded negative results. A second study was carried out involving a comparison of the effect of noise on CFF with monocular vision but with and without the artificial pupil. A partial replication of Ogilvie's results were obtained without the artificial pupil. These results seem to require caution in theorizing about possible neural mechanisms for the sensory interaction thus demonstrated.

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S. HOWARD BARTLEY, *Principles of Perception*. New York: Harper and Brothers, 1958. Pp. 482. \$6.50.

Here is another book on perception, an important textbook. There has been a fortunate revival of interest in the experimental psychology of perception, and several texts and syntheses of researches in this field are becoming available. Several more are said to be in the preparation stage.

In this new text Professor Bartley follows in a somewhat predictable way his well known book on vision, but he now gives us a bit more of his personal point of view regarding certain systematic issues in psychological science. The book is clearly a textbook and with certain necessary superficialities admitted by its author. Even with this primary objective, not uniformly achieved, there are many things of interest and of value to the professor and to the professional. There are some disappointments too, but the strength of these will of course depend on what you want and expect to do with this book and what you want and expect it to do for you as a source book and/or as a textbook for your students.

The volume consists of twenty-one chapters, including material on definitions, history and theory, phenomena and principles, and with the two terminal chapters titled "Perception in Everyday Life" and "Conclusions." Included are eight chapters devoted to strictly sensory processes. Because of the minimal uses made of this sensory material in the treatment of the strictly perceptual topics, there appears to the reviewer to be no special or important new contributions from this combination.

Some psychologists, but not all of us, wish to emphasize to their students that there is no real differentiation between sensory and perceptual experience or phenomena. They would possibly turn to Bartley's book for its combination. However, this very combination without integration may be disappointing as a text. Space limitations seem to prevent a well rounded presentation of sensory phenomena and processes—at least as full as we can find elsewhere for our students. On the subjects usually considered as perceptual in nature there are also some brevities and blanks. One very real lack, as far as this reviewer is concerned, is in the indexing of this book. Although treated, albeit briefly, important terms such as the following are not indexed: acuity, psychophysics, time perception or time error, albedo, olfactometer, phi phenomenon, saturation, satiation. A good text should be well indexed.

What about the utility of this text in most of the academic courses for which it may be considered? In an elementary course in "sensation and perception" it may be a natural. If the course is advanced, and especially if it is a graduate course, more extensive material will be required than is provided in Professor Bartley's book.

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SIDMAN, MURRAY. *Tactics of scientific research: evaluating experimental data in psychology*. New York: Basic Books, 1960.

Murray Sidman has written a book which Howard H. Kendler should read. In his chapter on "Learning" in the *Annual Review of Psychology* (1959), Kendler included a section on "Skinner" in which he opined that:

Skinner is an enigma. His attitudes, and those of many of his partisans toward facts, theories, statistics, and science itself are incongruous. But in spite of this his work, as well as that of his rapidly growing legions of adherents, is fascinating and creative, and has much of permanent value.

How does one evaluate the contributions of the Skinnerians? One can accept their framework of science and simply but enthusiastically list the facts discovered in the past year *in more or less random order* [italics added]. Or one can attempt to evaluate the contributions of the Skinnerians within a framework of science that is alien to them. Both will be attempted, with the realization that there is little common ground between the philosophy of science underlying Skinner's system and that of other contemporary theoretical orientations (p. 59).

Sidman's *Tactics* is a systematic exposition of the experimental philosophy and methodology which characterizes Skinner and his adherents. But, though *Tactics* may best be described as a detailed statement of the philosophy of science and approach to experimentation represented by the Skinnerians, it would be grossly unfair to Sidman to portray his contribution as any kind of "restatement" of what Skinner has already said. Since 1938, when Skinner's *The Behavior of Organisms: An Experimental Analysis* provided a formal presentation of his conception of a science of behavior, there has been tremendous growth in the experimental technology associated with the application of free-operant response reinforcement contingencies to the analysis of behavior in individual organisms. During recent years Sidman has played an important role personally in that growth, and *Tactics* has that growth as its foundation. At the same time, Sidman's book is a natural outgrowth of the point of view developed by Skinner in the "B. of O." and subsequent publications, so that as Sidman acknowledges in the Preface, "My debt to B. F. Skinner will be evident to the reader . . ."

The publication of *Tactics* seems to this reviewer to be particularly appropriate at this stage in the development of a science of behavior, which may account, in part, for its having been written. The views expressed by Kendler are representative of those of many psychologists, and the existence of such views indicates a serious lack of communication between Skinnerians and non-Skinnerians. Since Sidman states in considerable detail the Skinnerian attitudes toward facts, theories, statistics, and science itself, thereby making possible better understanding on

the part of the non- and possibly even the anti-Skinnerians, it is convenient to take Kendler's views as the starting point for this review.

Kendler is disturbed by Skinner, Skinnerians, and Skinnerian behaviorism. On the one hand, there is in it somehow work which is "fascinating and creative, and has much of permanent value." Skinner himself had published "two major works" in one year, and there are "rapidly growing legions of adherents." At the same time, these psychologists have attitudes toward "facts, theories, statistics, and science itself [which] are incongruous" and for whom there is "little common ground between the philosophy of science underlying Skinner's system and that of other contemporary theoreticians." Therefore, "How can so many important experimental techniques and interesting findings spring from such an anti-theoretical attitude?" Kendler has a theory, of course, to account for this: "One contributing factor is the sheer genius of Skinner himself. This writer suspects that if it had been possible for Skinner to have applied his restless and brilliant intellect to the revival of structuralism, he would have been eminently successful." This explanation is not fully sufficient, however, behavior being multiply-determined, so that another variable must be included: ". . . the fascination many Skinnerians have with the idea of controlling behavior. This has led them directly to potent psychological variables," an opinion which might be interpreted as implying that by adopting alternative, non-Skinnerian tactics, Skinnerians could have (a) been led directly to less potent variables, or (b) gone only indirectly to potent variables. "But it would be both foolish and naive to attribute the successes of the Skinnerians to their antitheoretical, antiquantitative and antiresearch design positions," Kendler warns, and offers a kind of Freudian account to the effect that in reality ". . . the Skinnerians have indulged in covert theorizing even though they refuse to recognize it."

Referring to the "work of Sidman (who gets this year's prize for the highest publication response rate)," Kendler notes that, "In one paper Sidman, Herrnstein, & Conrad (1957) write: 'It has been our experience that an experimental design in which each step depends on current data is more economical and productive than a preconceived and fixed plan of experimentation.'" To Kendler "This 'option-play' (or 'playing-by-ear') type of research generates a mass of data that is most difficult to analyze systematically," but then what can you expect of investigators for whom "Each experimental article contains many more variable than subjects."?

In *Tactics*, Sidman explains and justifies these research practices. The scope of Sidman's undertaking is stated in the opening paragraph:

There are three questions of paramount concern in evaluating experimental findings: (a) the scientific importance of the data; (b) their reliability; and (c) their generality. These considerations are by no means

independent of each other, but for the sake of convenience I shall treat them here as though they were separable (p. 1).

The book contains four parts: Part I. Guideposts for Evaluation (1. The scientific importance of experimental data, 2. The reliability and generality of data); Part II. Replication (3. Direct replication, 4. Systematic replication); Part III. Variability (5. Intrinsic versus imposed variability, 6. Variability as a scientific and as an engineering problem); Part IV. Experimental Design (7. Pilot studies, 8. Steady states, 9. Steady states continued, 10. Transition states, 11. Selection of an appropriate baseline, 12. Control techniques, 13. Control techniques continued). There is also an "Appendix—A Terminological Note" which this reviewer recommends to most readers as an Introductory Note.

Concerning the scientific importance of experimental data, Sidman concludes his discussion by stating that history is the prime evaluator, not the theory allegedly guiding the experimentation. This does not free the investigator from trying to make intelligent choices about the variables in which he will invest his efforts, but does free him from the delusion that there are "impartial rules within which the scientist can operate when he has to make his evaluation" (p. 41). For it is "The cumulative development of a science [which] provides the only final answer as to the importance of any particular data," and the young scientist should beware the unfortunate truth that

Some scientists either never realize at all that they are sitting on a shaky limb, or they deny that realization and claim that *their* feet are firmly on the ground. They do not hesitate to condemn a colleague's work as unimportant, for to them importance is, by definition, circumscribed by their own set of rules. The position provides security, but it does so at the expense of the objectivity that is the first requisite for effective science (p. 41).

Concerning the reliability and generality of data, a similar theme prevails, since

As was the case with the importance of data, the assessment of reliability and generality requires maturity of judgment. There are, however, many psychologists who *insist* that reliability and generality be evaluated on a purely impersonal basis . . . possibly due to [their] recognition . . . of the frailties of human judgment . . . But individual judgment is by no means synonymous with individual whim . . . The objectivity of science consists not so much in set rules of procedure as in the self-corrective nature of the scientific process (pp. 42-43).

There follows a discussion of reliability and generality which should be required reading for all undergraduate and graduate students and instructors of courses involving statistical methods, the tenor of which is indicated by these remarks:

. . . Are experimental observations ever the result of chance? To some experimenters, chance is simply a name for the combined effects of uncontrolled variables. If such variables are, in fact, controllable, then chance in this sense is simply an excuse for sloppy experimentation, and

no further comment is required. If the uncontrolled variables are actually unknown, then chance is . . . a synonym for ignorance. Science is presumably dedicated to stamping out ignorance, but statistical evaluation of data against a baseline whose characteristics are determined by unknown variables constitutes a passive acceptance of ignorance . . . a curious negation of the professed aim of science. More consistent with those aims is the evaluation of data by means of experimental control . . . (p. 45).

Generality is treated in relation to subject generality, or representativeness; interspecies generality; generality of variables; generality of process; and methodological generality. Throughout this discussion as elsewhere in his text, Sidman emphasizes the concept that scientific data are evaluated by humans, with the evaluations reflecting the context in which the evaluator is operating, and, therefore,

The weight to be assigned to any given change in the experimental conditions [with respect to evaluating the generality of data obtained from different conditions] will depend upon both the general state of existing knowledge in the scientific area in question and the acquaintance that any particular scientist has developed with respect to that area. The degree of confidence that prevails in a scientific community with respect to any particular induction will, therefore, be a function of the extent to which the members of that community share a common history of experience [in relation to the data being evaluated] (p. 61).

For *experimenting* psychologists genuinely interested in improving their own research tactics, the two chapters on direct and systematic replication—which constitute the heart of Sidman's conception of the methodology most appropriate to evaluating reliability and generality—offer much of value, dealing as they do with problems such as the replication of behavioral phenomena within individual organisms, the assessment within a single organism of the control exerted by different variables, and the determination of parametric functions within the individual organism.

For psychologists in general, it is my opinion that the section on variability is the most important single part of *Tactics*. There seems to be growing acceptance of the experimental technology generated by the Skinnerians (even Kendler offers praise here). But the heart of the objections to the Skinnerian approach seem to involve two assumptions: one, that without "theory" a systematic science is not feasible, and two, that organisms are characterized by "intrinsic variability" such that only traditional research designs and statistical tools are appropriate.

With respect to the theory that theories are a necessary condition for the conduct of meaningful research, its advocates fail to recognize that the term is not a primitive one: the definitions assigned it by various psychologists are not sufficiently equivalent to permit us to use the term without first defining it. Any selective definition of theory must distinguish that term from hunches, opinions, attitudes, speculations, hypotheses, and conglomerations of these. Otherwise, the term theory becomes so all-inclusive as to conquer itself to death. To in-

sist that a psychologist cannot do research without a theory in the sense that he must be behaving in terms of hunches, opinions, speculations, etc., is to say no more than that he is a living human organism. To suggest that "covert" theorizing accounts for the fruitfulness of Skinnerian experimentation is hardly helpful. Covertly or overtly, Skinnerians are not bound to some anti-theory oath. Their opposition is directed at premature, inadequate, or "miniature" theories. Theory-construction, as Sidman points out, is an arduous, demanding task, and hardly one to be undertaken until a sufficient array of data is on hand to warrant the effort.

The question of "intrinsic variability" and "individual differences" is a different type of obstacle to progress in psychology. Having a particular theory may not necessarily seriously handicap an investigator's efforts to learn about the nature of behavior, as long as the discovery of "potent" controlling variables serves to reinforce his scientific behavior. Pratt (1948) commented in a footnote that,

Yet, countless psychological experiments, so-called, are done today by means of a sort of shot-gun method. A complicated stimulus set-up, such as a picture, or a paragraph heard from a loud speaker, is presented to observers who are merely told to report what they see or hear. Almost anyone would know that under such circumstances the observers would hear or see different things. It takes psychologists to be impressed by such hackneyed stuff and to talk learnedly about its subsumption under a *theory* of individual differences (p. 101).

Today comparable experimentation persists in both the animal and human laboratory, though a theory of individual differences has been replaced by the exceedingly more dangerous dogma that statistical techniques can locate truth despite third-rate experimental control. Individual variability is obvious, Sidman states, thus reassuring those who believe that essentially "religious" values should dictate the form of behavioral laws. But, he continues,

The underlying thesis has been that variability is not intrinsic to the subject matter [i.e., behavior] but stems, rather, from discoverable and controllable causes. The door was left open for the admission of intrinsic variability [e.g., scientifically legitimate individual differences], but only after, and if, a high degree of technical development and systematization of data has forced it upon us (p. 191).

Sidman then draws a distinction which the young psychologist committed to a research career should consider, a distinction between scientific and engineering problems in psychology:

The process of systematically tracking down sources of variability, and thus explaining variable data, is characteristic of the scientific enterprise. Variability, however, may have different implications for the investigators we may call behavioral engineers . . . whose work is concerned with intelligence and aptitude testing, man-machine interactions, behavioral therapy and diagnosis, opinion sampling and control, and related aspects of applied psychology . . . The behavioral engineer must ordinarily take variability as he finds it, and deal with it as an unavoidable fact of life . . . The behavioral engineer seldom has the facilities or

the time that would be required to eliminate variability. We may sympathize with his plight and admire his achievements in the face of such difficulties. Some basic scientists . . . go further than admiration. They . . . attack basic scientific problems as if they were engineering problems. Hence the insistence upon large samples and statistical criteria of generality, stress upon subject generality rather than upon the generality of principles, and the resigned acceptance of intrinsic variability (pp. 192-193).

Sidman's evaluation of those who treat basic scientific problems as engineering problems may seem unduly harsh, for "The basic scientist . . . is failing in his obligation when he treats his subject matter as if it were an engineering problem. When he orients his experimental techniques around a fatalistically accepted conception of intrinsic variability, he forfeits his right to the investigative luxuries of fundamental science" (p. 194).

Since a considerable number of the non-Skinnerian psychologists who may find Sidman's views of value are probably those currently working on problems in the area of "learning," it is pertinent to mention his concept that learning—as a state of transition between two stable states—requires for truly systematic analysis more effective experimental attention to the original stable state and final stable state which function as baselines for defining the transition process. The traditional learning psychologist, having accepted the concept of behavioral irreversibility as a kind of derivative of intrinsic variability, has necessarily been handicapped by his resistance to analyzing learning in single organisms. Sidman suggests—and hopefully his foes will seek data with which to assess the validity of his suggestion—that by attaining better experimental control over the pre-learning baseline, learning-oriented psychologists might bring about a new era.

This reviewer (who is, it must be admitted, an ardent adherent of things Skinnerian) had three main complaints. First, it would have been extremely valuable if Sidman had taken the time to formulate his concept of theory, and to describe what he would consider an acceptable theory in the field of behavior. Second, it would have been helpful if Sidman had devoted some attention to the rationale underlying the use of free-operant procedures and rate of response measures: why rate and related indices are taken as most appropriate to the search for controlling variables. Since there seems to be every reason for assuming that both "free" and "controlled" operant procedures will continue to be basic tools in the hands of experimenting psychologists, and since, as a result, we will continue to be hampered by the problem of coordinating data involving either rate-related measures or latency and choice-frequency measures, a systematic experimental coordination of "free" and "controlled" operant techniques is sorely needed. Sidman might have provided at least some hints as to how we might treat this quasi-schizophrenic state of contemporary experimental psychology. Third, though *Tactics* includes Sidman's classic demonstration of concurrent suppression of food-reinforced and facilitation of avoidance re-

sponding within the context of the now standard "conditioned anxiety" or "conditioned emotional response, C.E.R." paradigm, it does not, unfortunately, include two recent investigations—one by Hearst¹ on the analysis of the relation between number of reinforcements and extinction responding within individual subjects and one by Stoddard, Sidman, & Brady² on a parametric analysis within individual subjects of "switching" behavior in "normal" and schizophrenic subjects—which, to this reviewer, constitute two of the most important illustrations of the power of the tactics espoused by Sidman.

Whatever its deficiencies, Sidman's *Tactics* is a major contribution to the science of behavior. As such it is a genuine tribute not only to its author, but also to the historical variables which enabled him to emit it (e.g., Skinner, Keller, Schoenfeld, Brady, *et al.*).

Bryn Mawr College

DONALD H. BULLOCK

¹ Eastern Psychological Association Meeting, 1960.

² Psychonomics Society Convention, 1960.

The Psychological Record, 1961, 11, 199.

Spence, K. W. *Behavior theory and learning*. Englewood Cliffs, N. J.: Prentice-Hall, 1960. Pp. 403.

Beach, F. A., Hebb, D. O., Morgan, C. T. and Nissen, H. W. (Eds.) *The neuropsychology of Lashley*. New York: McGraw-Hill, 1960. Pp. 564.

Peatman, J. G. and Hartley, E. L. (Eds.) *Festschrift for Gardner Murphy*. New York: Harper, 1960. Pp. 411.

These three volumes are part of what appears to be a trend in publishing. Various "collections" are appearing; among the most useful are those of the "selected papers" variety of which the first two volumes are representative. They are essential when library journal facilities are inadequate since many individuals whose work merits such treatment have utilized a variety of publications, including foreign ones. It is certainly convenient to have a man's papers published over a span of years, bound together and for those who would enjoy personalizing articles with marginal notes this opportunity is finally possible. There is perhaps no better way to observe the development of a man's thought, the breadth and changes of his interests, etc. They seem far more useful than the *Festschrift* approach represented by the third title where the essays have little more in common than the attempt of a man's former students to honor him. As individual papers each may have merit for the specialist. The first two titles serve a scholar's needs better, although their price of about nine dollars each is prohibitive for personal libraries.

Denison University

IRVIN S. WOLF

The Psychological Record, 1961, 11, 200-202.

BUCKLEW, JOHN. *Paradigms for psychopathology, a contribution to case history analysis*. Phila.: J. B. Lippincott Co., 1960. Pp. xii + 236.

This is not "just another text in abnormal psychology." Even before proceeding through the contents of this volume, a look at the title tells us that it is going to be different. In the introduction, Professor Bucklew states that the book is devoted to "bringing some semblance of order out of the welter of recorded facts, interpretations, and special terminology which characterize the case-history method." In order to do this, he has attempted through the use of paradigms and diagrams to visualize for the reader the important relationships which exist in a single case-history. Admittedly, this is not the first attempt ever made to use diagrams to show relations between psychological events. Others have tried with varying degrees of success. Certainly, this particular approach is original and unique.

The author states that the basic unit of action is the stimulus-response which through the process of behavioral development becomes incorporated into larger units of action which can be handled as functionally integrated as well as independent entities. Examples of these units include the ego, the complex, anxiety as well as such dynamic processes as motivational conflict, repression, fixation, and regression. At this juncture one might wonder within what theoretical framework Mr. Bucklew is working. He acknowledges three main sources for his ideas: the interbehavioral psychology of J. R. Kantor, psychoanalysis of Sigmund Freud and the ego psychologies (Allport, Sherif, et al). One is bewildered at the onset as to how such opposing viewpoints can be reconciled. The answer one finds after reading the book may not be too satisfying to some, particularly to those with a strong feeling toward a molecular analysis as exemplified in a bar-press or key-peck. The acceptance of the system or its rejection depends on one of the basic postulates, namely, that more complex units of behavior (other than the S-R) come into existence as a result of psychological development. Behavior then becomes integrated into larger functioning wholes making it necessary, according to Bucklew, to describe their operation at a different level from the isolated S-R unit. Of course, the constructs on which the paradigms are based could not be used otherwise.

The constructs, at least in name, are familiar to anyone working in the field. The author has attempted some redefinitions of them both verbally and visually. They serve as the bases for the general models for diagramming the various types of abnormality. Through the use of such a systematic approach, an understanding of abnormality can hopefully be achieved along with the possibility of predicting symptom formation when certain prior events are known. Following the introductory chapter in which the basic postulates taken from general psychology are stated, a brief chapter is devoted to an argument favoring the case history as a scientific document, since it is the datum on which

the analysis is built. The third chapter, "Constructs and Diagram Elements" is the most crucial to an understanding of the rest of the text. It is here that the basic constructs are defined and the diagrammatic elements explained. This chapter concludes with a section on "The Experimental Basis of Constructs." Here some of the familiar literature is reviewed on such subjects as repression, regression, fixation, conflict, anxiety and the ego. The author concludes that some of his constructs (motivational conflict, anxiety, regression) for example, have parallels in experimental work with animals, but others (such as role, ego, repression, traumatic events, external conflict) do not have any clear parallel. He suggests the existence of these latter processes in an organism who has been socialized in a human culture and, therefore, they are more or less restricted to human beings. This reviewer would take exception. For example, in the experimental work on punishment by Estes, Skinner and others one finds good parallels at the animal level for what we commonly call repression. Or what about Dollard, Miller and Mowrer, to name a few, on external conflict? No one is arguing, least of all Mr. Bucklew, that studies of "experimental neurosis" as demonstrated in dogs, cats or what have you are all that is necessary in order to gain an understanding of human abnormality.

The remaining chapters (except for the last) are devoted to an analysis of the paradigms and diagrams for neurosis (obsession-compulsion is a good example), the inadequate personality and the psychopathic personality. Modes of abnormality (excluding psychoses) may be thought of as a continuum running from extreme intrapersonal conflict in the neuroses to extreme interpersonal conflict in the psychopath with the inadequate personality falling in the middle. Such an interpretation assumes that one is willing to accept the existence of intrapersonal conflict and is willing to distinguish between the two. The implication that some conflict is "inner" is going to be bothersome to those psychologists who feel they have been able to dissociate themselves from the ideas expressed in the writings of Freud, Horney and the like and prefer a more operational approach. Although a chapter is devoted to diagrams for various psychotic reactions, it is noted that individual psychoses differ so greatly that no true paradigm for each separate diagnostic classification can be drawn. Bucklew does suggest, however, some ways of diagramming cases of paranoid schizophrenia, schizophrenic deterioration and one for a depressive psychotic reaction.

The final chapter is devoted to an excellent discussion of abnormality and value systems. Here a pragmatic basis for distinguishing abnormal from normal personality is presented, as opposed to a statistical one. In the pragmatic distinction, the abnormal personality is one who has become so disrupted and inefficient that he no longer meets the major requirements of society. To try to explain abnormal in terms of conformity vs. non-conformity, is to substitute a value judgment for an understanding of the problems. Furthermore, the distinctive criterion of maturity vs. immaturity pushes the discussion into the social ideals

of what proper conduct ought to be, so value judgments become substituted for explanations.

As this reviewer pointed out at the beginning, this is a unique book; unique in many respects. It is a short book (236 pp) when compared with the current list in the field. It is not intended to have the general coverage of a basic text, but as the author suggests, it can be used as an adjunct to a standard text or part of a course in which no single text is used. The bibliography is likewise short (only 112 titles), many of which refer to experiments discussed in Chapter III. However, these are not meant as negative criticisms for in an approach as original as this, a long bibliography of extended discussions of syndromes would serve no useful ends.

As we strive towards a more scientific psychology in these days where mentalism floods the field of abnormal psychology, there are times when one is much heartened by this approach. It strives to treat psychology in a scientific manner and this is most commendable. The task is a difficult one and the use of the case history is certainly more difficult to treat scientifically than a simple animal experiment where the relevant variables can be appropriately manipulated. Certainly the study of human abnormality is also a most complicated one and many a person's symptoms are often verbal or even implicit. This behavior can hardly be explained at the animal level. How successful he has been in his efforts can only be judged on the basis of whether or not one is willing to accept some of the basic assumptions regarding the development of the ego, complex and dynamic relationships. The author does not want to be non-operational, but some of his constructs, at least to this reviewer, would fail a test of operational validity. Consider some of these statements. "Generally speaking ego is nothing other than self-management." (p. 36), or "Ego alien motives are ones which have been repudiated or disowned by the person and merely have not achieved any degree of integration with the rest of personality." (p. 41), or "Such elements of personality (cultural personality traits) should not be considered repressed because they have not become unconscious through conflict." (p. 38). One wonders if the ego is still the "little man up there" who tells us what to do or what not to do. What is meant by "unconscious" in this context? One of the great difficulties with Freud and the psychoanalysts from the viewpoint of an objective psychology, is that his constructs are untestable. Furthermore, his constructs of the ego, id, and super-ego become so fictional that one must ask, what happened to the basic data? Although one does not wish to accuse Mr. Bucklew of falling into the mentalistic trap, this present reviewer wonders if his constructs have not gotten too far away from the basic data. Perhaps this is permissible if one is willing to acknowledge that the case history is a legitimate scientific protocol, but in the light of recent experimental findings one should not build a system of explanation on the case history alone.

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ROBERT W. LUNDIN

The Psychological Record, 1961, 11, 203.

LEWIN, B. D. and ROSS, HELEN. *Psychoanalytic Education in the United States*. W. W. Norton & Co., Inc., New York. 1960. xviii, 478.

No word in our present American vocabulary is so loosely bandied about as "psychoanalysis." In England the laxity was such that the British Medical Association conducted a three-year investigation of legitimate psychoanalysis, and turned in an extremely favorable report (1929). The present book is a summary of a different sort of evaluation, but of the same subject. An analyst and an executive secretary of a large training institute took three full years to inspect (and introspect about) the psychoanalytic training process in this country. Their report covers only the fourteen institutes and three training centers, and is concerned with two tasks: the collection of information about such diverse matters as the sometimes mysterious process of candidate selection, age of these students, cost of training (fantastic), various details of educational policy and the like; then, interspersed with charts and graphs, the more generally psychologically interesting observations on the entire approach to the post-post-graduate problem itself (most candidates have already an M.D., and often their specialty boards in psychiatry). Many interesting parallels and differences are noted between this and the doctorate program in the various sciences. Of particular interest is their turning of the psychoanalytic spotlight upon the problem of the indefinite prolongation of 'graduation' from either area. One entire section of this problem clearly points out the need to differentiate between education process, and the actual maturing factors, the latter often being masked (in the service of feet-dragging) as academic ennui, love of 'Cambridge,' or as based on actual psychiatric problems. Psychologists who are immersed in the problems of graduate student selection, training and graduation will be pleased and amazed at this essentially new and intensive approach to the problems they must face, and to a large extent, must solve intuitively. It is refreshing to see the spotlight turned inward.

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RAYMOND HEADLEE, M.D.

The Psychological Record, 1961, 11, 204

Solley, C. M. and Murphy, G. *Development of the perceptual world*. New York: Basic Books, 1960. Pp. xi + 353.

The present volume reminds this reviewer of Beach's chapter in Stevens' *Handbook*, in which it was pointed out that instinctive behavior, when investigated experimentally, was found to be related to experimental variables in a manner similar to that already found in the case of non-instinctive behaviors. Perception is ripe for theoretical union with other behaviors.

In view of the authors' disclaimer concerning the adequacy of their own theorizing, it is difficult to be critical of this feature of the work. The inadequacy of the theory presented is a natural consequence of their apparent failure to appreciate the postulational nature and basis of modern theory construction. Their naivete concerning postulation is further attested by a complete lack of understanding that it is possible to be "operational" without being a "logical positivist." The volume presents internal inconsistencies which may indicate either inadequate editorial work or difficulties inherent in multiple authorship.

Certain features of the volume raise worthwhile questions. Solley and Murphy attempt to reconcile the historically prominent viewpoints of *atomism* and a unifying principle. Beginning with Kant, many eminent thinkers have attempted this reconciliation, and failed. These two viewpoints may be inherently contradictory. The perceptual act is stated to be self-reinforcing. Does such a concept tend to remove the perceptual act from investigation, or does it open broad new vistas for investigation?

The reviewer can only concur with the authors' statement that: "If this book stimulates further research, we shall judge it a success . . ."

Denison University

PAUL T. MOUNTJOY

FULLER, JOHN L. and THOMPSON, W. ROBERT. *Behavior genetics*. New York: John Wiley & Sons, Inc., 1960, p. 396. \$8.95.

In attempting to evaluate *Behavior Genetics*, two points stand out sharp and clear for this reviewer. First, the authors admit a "difficulty . . . in selecting suitable materials for citation, especially when these fell short of exacting scientific standards. This has been true of studies dealing with human beings. We considered omitting human studies completely, but did not do so because of the primary interest of many readers in human problems" (Preface, p vi). Had the authors

decided to exclude the data on humans, both the size of their volume and its potential significance would have shrunk considerably.

A second point involves a still more fundamental criticism. The authors start with a "silent assumption" implicit in the following quotation: "An infinite number of measurements may be made upon the body of an organism or upon his behavior. *In a sense* (italics added) each of them may be considered as a character whose inheritance may be studied. . . . Traits which have been utilized in behavior genetics range from specific motor components of fish courtship to susceptibility to perceptual illusions and scores on Stanford-Binet tests" (p. 4). A trait includes "agglutinogens of red blood cells," "customs and language," or "skin color" and "body size" (p. 2), yet the term is never defined.

If one starts with the premise that, "in a sense," a tropistic response and a score on a Stanford-Binet test are comparable, equivalent, or identical, then from there on, one must follow wherever the theoretical maze may take him. But in *what* sense is a score on a test to be equated with a developmental anomaly or a smooth or wrinkled pea? This reviewer suggests that the unwitting framework within which *Behavior Genetics* was written is a reductionistic one in which behavior, which is action, is treated as if it were an anatomical or physiological characteristic of an organism. This is the fundamental point at issue.

However, one always has theoretical choices because data do not come with their proper understanding engraved upon them. Since Fuller and Thompson start out with the constructional alternative that requires no distinction between the data of psychology and the data of biology, their outcome is, in a sense, predetermined. But with what results? After 316 pages, one comes away with the realization that "the exact nature of the relationship between nature and nurture in the production of individual differences is, however, by no means settled" (p. 317). But, accepting the theoretical position under which Fuller and Thompson operate, then one must acknowledge their earnest, conscientious and exhaustive labor of love. With its coverage of more than 500 references, the book succeeds in accomplishing the authors' aim of presenting current thought on the three problems: "What are the effects of heredity upon behavior? How large are these effects? What mechanisms are involved?"

University of Wichita

N. H. PRONKO

The Psychological Record, 1961, 11, 205-206.

VIAUD, GASTON. *Intelligence, its evolution and forms*. New York: Harper & Brothers Publishers, 1960, 127 pp.

Beginning with Bergson's idea of *practiced intelligence* viewed from an evolutionary perspective, the author divides intelligence into

two forms or types: 1) the lower form of practical intelligence and 2) the higher type known as logical or rational intelligence. The former is found in animals, children, and adults and is used to solve problems more primitively via the use of movements and actions for dealing with objects and events. In contrast, logical and conceptual intelligence involves the application of abstract notions to facts.

This reviewer is of the opinion that Viaud's monograph does not add one iota of scientific understanding of the behaviors that have been conventionally and casually labelled with the term, "intelligence."

University of Wichita

N. H. PRONKO

The Psychological Record, 1961, 11, 206.

BIRNEY, ROBERT C. and TEEVAN, RICHARD C. *Instinct*. New York: D. Van Nostrand Company, Inc., 1961, 181 pp.

TEEVAN, RICHARD C. and BIRNEY, ROBERT C. *Color vision*. New York: D. Van Nostrand Company, Inc., 1961, 214 pp.

BIRNEY, ROBERT C. and TEEVAN, RICHARD C. *Reinforcement*. New York: D. Van Nostrand Company, Inc., 1961, 230 pp.

The authors have achieved their goal of showing students how "theories and researches build on what has gone before; that one study leads to another, that theory leads to research and then to revision of theory." They have, likewise, rendered psychology instructors a real service in arranging in a historical and logical sequence classics in each of the three areas within the compact confines of a pocketbook.

Color vision starts with Thomas Young's papers, runs through Helmholtz, Hering, Selig Hecht and winds up with selections from Clarence Graham, Edwin H. Land and Charles E. Osgood.

Instinct reproduces William James' article on Instinct, continues with original papers from Kuo, Carmichael, Lorenz, Lashley, Hess, and Tinbergen, and concludes with more recent papers by Lehrman and Beach.

The *Reinforcement* volume ranges from papers by Thorndike, Hull, and Guthrie to late ones by James Olds and Neal Miller.

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N. H. PRONKO

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CONTENTS

An analysis of the Decision-Making
Functions of a Simulated Air Defense Direction
Center.....*by Anders Sweetland and William W. Haythorn*

Instinct and History.....*by Nicolas Malleson*

A Model for Equisection Scaling.....*by Robert F. Fagot*

Models for Medical Diagnosis.....*by John E. Overall and
Clyde M. Williams*

A Model for the Comparison
of Individual and Group Performance
Scores.....*by Ivan D. Steiner and Nageswari Rajaratnam*

Critique and Comment
The Leviathan Technique for Large-Group
Analysis.....*by Sydney C. Rome and Beatrice K. Rome*

Some Remarks on the Whorfian
Hypothesis.....*by Gerald A. Radnitsky*

Book Reviews

Computers in Behavioral Science
Preparing Psychophysiologic Analog Information for the
Digital Computer.....*by Herbert Zimmer*

Automated Programming for Multinomial Distribution
Problems on the IBM 650.....*by James E. Farmer*

Computer Program Abstracts (No. 39-46)

Abstracts of Current Literature with Interdisciplinary Implications
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